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A REPLY TO PROFESSOR MARSH'S "NOTE ON MESOZOIC MAMMALIA."[1]

BY PROF. HENRY F. OSBORN.

"OF more than a thousand specimens of Cretaceous mammals on which my investigations are based he has not seen a single one, and no others are known, except a few fragments."

In the above paragraph Professor Marsh refers to the fact, also stated very distinctly in my review of his "Discovery of Cretaceous Mammalia,"[2] that I had not examined his material, and implies that my criticism is of less value because based solely upon his descriptions and figures. This, I may explain, was not from any lack of appreciation of the importance of studying type-specimens, but because these types, together with the other large mammalian collections belonging to the government, are not accessible to American paleontologists. I should certainly have examined them if I could have done so under conditions[3] similar to those in which specimens belonging to the various foreign museums are made accessible to others and myself.

[1] This "Note" is a reply to an article entitled "A Review of the Cretaceous Mammalia," by Henry F. Obsorn. Proc. Acad. Nat. Sc., Phila., 1891; and was read before Geological Section, A. A. A. S., Washington, D. C., August 21st, 1891.

[2] The original unaltered copy of "Note on Mesozoic Mammalia," as privately printed and distributed abroad, was reprinted in the AMERICAN NATURALIST for July, with the original copy of the "Review." For the amended "Note" see Proc. Acad. Nat. Sc., Phila.

[3] In former papers I have made full acknowledgments of the opportunities afforded me by Professor Marsh for examining his types. Such were also extended to me in connection with the Cretaceous mammals, but under conditions which I could not accept.

Without question, these and all other U. S. Geological Survey types, now that they have been described and figured by the author, should be placed in the National Museum, where they could be studied under the ordinary museum restrictions. The examination of type specimens which the author regards "as a matter of courtesy" is rather a matter of custom and necessity; especially is this true where the types are not private but public property, and have been employed in official reports.

Nevertheless, after carefully reading the author's "Note," I am more confident than before that an examination of the types will fully confirm all the main points raised in my review. As the "Note" contains very little in the nature of direct consideration of my criticisms, I had not intended to answer it; but my attention has been called to the probability that, like the author's original papers on the Cretaceous Mammalia, this one might have considerable weight with naturalists who happen to be entirely unfamiliar with the morphology and literature of the Mesozoic Mammalia. We find two features in the "Note": first, a number of definite statements in regard to the structure and classification of the Mesozoic mammals which bear directly upon this discussion,—these I shall now consider and answer; second, a number of unfavorable but indefinite comments upon the character and methods exhibited in my paleontological papers. To the latter I shall make no reply, because, if these papers are of no value, my personal statement to the contrary would have no weight; moreover, such discussion would merely divert attention from the real question at issue, as raised in my review,—viz., the character of the evidence advanced by Professor Marsh for the existence of a very numerous and highly varied Cretaceous fauna. Of the twelve specific statements made by the author in his "Note," four are in defence of his "Cretaceous Mammals," eight are in criticism of alleged errors in my papers upon the Jurassic and Triassic mammals, especially in the "Memoir Upon the Structure and Classification of the Mesozoic Mammalia." I may first reply to the latter.

1. It is first stated that I separated Dromotherium from Microconodon upon insufficient grounds, and figured the type of the

latter genus inaccurately.[4] I would say, in reply, that Microconodon is entirely distinct from Dromotherium in the structure both of the teeth and jaws, excepting only in the imperfect division of the molar fangs, which in this instance is not a generic but at the least a subordinal character. The figure of *Dromotherium sylvestre* was drawn with the utmost care, yet I will be glad to correct any inaccuracies if the author will kindly point them out.

2. That I attributed a pineal foramen and eye to Tritylodon, and later was compelled to retract it. The facts are that in a note to *Science* I based this suggestion upon Owen's figure and description of an apparent "fontanelle" between the parietals and frontals. At the same time I wrote to the British Museum asking Dr. Baur to make a careful examination of the skull itself, and, upon my learning that there was no foramen, I immediately published a correction in *Science*,[6] and moreover withdrew the suggestion in my memoir.[5]

3. That my figure of Phascolotherium was inaccurate in four important points. In reply, I may say that three years ago I published[7] a correction of the only error in this figure,—viz., the elevated position of the dental foramen. In regard to all three other inaccuracies the author of the "Note" is mistaken: 1. As may be seen by reference to Flower & Lydekker's recent work,[8] the first incisor is present; 2. The mylohyoid groove is correctly figured, as may be seen by comparison with figures published by Owen and others; 3. As stated in the explanation of the plates, the last molar was restored from Dr. Buckland's figure,[9] which was made before this tooth was detached and lost,—a perfectly legitimate proceeding.

4. That I misquoted the title of "Discovery of Cretaceous Mammalia." This is the only point in which the author is entirely correct. I find that inadvertently the article "the" was included in quotation marks.

[4] "Mesozoic Mammalia," p. 222.

[5] "Mesozoic Mammalia," p. 220, foot-note.

[6] "No parietal eye in Tritylodon," *Science*, 1887, p. 538.

[7] Proc. Acad. Nat. Sc., 1888, p. 294.

[8] "Mammals: Living and Extinct," p. 114.

5. That my definition[10] of the Multituberculata (1) omitted some of the characteristic features of this group, and (2) embraced accurately the genus Mastodon. I find that my definition included the only three distinctive and universal characters of this group which were known at the time (1888); in fact, no additional characters have since been published, nor are any mentioned by the author of the "Note." No one, except the author, could confuse my definition with that of the Mastodon, because the Multituberculata was placed as a suborder of the Marsupialia. The best criterion of the clearness and sufficiency of this definition is that the term defined has been universally adopted in subsequent official and standard publications (see Flower, Lydekker, Trouessart, Döderlein, Ameghino, and others).

6. That I overlooked the strong probability that the type of Bolodon is an upper jaw of a species of Plagiaulax, and made it the type of a new family. The fact is, that so far from overlooking the resemblance between Bolodon and Plagiaulax, I have given the fullest discussion which has ever been published[11] of the evidence for and against the union of these forms into one genus and family. The author, moreover, does not mention that the latest expression of opinion upon the subject is his own, and in favor of the very views he is here criticising, as shown in the following sentence: "These fossils evidently belong to the family named by the writer (*i.e.*, by Marsh) the Allodontidæ, which includes the American genus Allodon, and Bolodon from the Jurassic of England."[12]

7. That I have overlooked the probability that the type of Stereognathus is an upper jaw, although heretofore described as a lower one. Nothing can be said in support of such a probability. The universal opinion of English writers, following the exceptionally careful figures and descriptions of Charlesworth and Owen, is that this type is a lower jaw. I have not examined

[9] Op. cit., Fig. 29.

[10] "Mesozoic Mammalia," p. 213.

[11] "Mesozoic Mammalia," p. 217.

[12] A. C. Marsh, *Amer. Jour. Sci.*, p. 179. See also same journal, April, 1887, p. 329.

the type itself, but even since the author's suggestion[13] that the type is part of a maxilla it is again described and figured as a lower jaw in Flower and Lydekker's "Mammals."[14]

8. That I followed Cope's error in founding the genus Meniscoëssus upon a supposed premolar, which is, in fact, a reptilian tooth. It is distinctly stated in my review, as well as in Cope's original description, that the type of Meniscoëssus is the molar tooth, as is also implied by the term itself.[15] Professor Cope himself expressed serious doubts as to the mammalian nature of the "premolar." When I examined it, finding a basal cingulum and close histological resemblance to the molar, I described it as a premolar, and figured it as a probable premolar. There is, however, reasonable doubt as to its mammalian character, for the basal portion of the crown is entirely wanting, removing all evidence as to the character of the fangs.

9. That I mistook two portions of a fish (Hybodus) tooth, artificially cemented together, for a mammalian premolar. I concluded my description of this tooth in the following way: "If it is actually from the Rhætic beds, it probably represents a premolar of Triglyphus." I thus clearly expressed my doubts as to its reference to a long-established genus, and was far from selecting such a specimen as the type of a new genus and family of mammals, as the author has done in the case of Stagodon.

It thus appears that of these eight alleged errors I have myself long since published corrections of the two relating to Phascolotherium and Tritylodon; that nothing has been added by recent discovery to my definition of the Multituberculata; that my association of the supposed premolars with Meniscoëssus and Triglyphus was in each case accompanied by an expressed doubt, either in the description or figure; finally, that my conclusions regarding the Bolodontidæ (Allodontidæ), if erroneous, have nevertheless been adopted and supported by the author himself. The author's criticisms will therefore have little weight with persons who

[13] *Amer. Jour. Sci.*, April, 1887, p. 343.

[14] P. 110, Fig. 27.

[15] The question of this nomenclature is fully discussed in the AMERICAN NATURALIST, July, 1891.

are fully familiar with the literature of this subject.[16] Let us now consider the four points which the author has advanced in reply to my review of his determinations of twenty-seven distinct types.

1. First, as regards the types of the genus Stagodon and family Stagodontidæ, the author still considers them as mammalian teeth, and remarks: "I distinctly stated that this tooth has two fangs, and the bases of these were indicated in one of my figures." As this point is an interesting one, I reproduce for a second time the figures of the teeth which were selected as the

FIG. 1.—(STAGODONTIDÆ.) *a*, *Stagodon nitor*. AfterMarsh. Types. *b*, *Platacodon nanus*, After Marsh. Types.

types of Stagodon, and allow the reader to judge. We must direct our attention only to these types, remembering that the other teeth were all found isolated, and the fact, as urged by the author, that "other teeth described as premolars," and "well-preserved specimens since obtained" show distinct fangs, has no bearing upon the question, since the association of these teeth with the types is purely conjectural.

2. The order "Allotheria." The author claims that he has defined it, that it is prior to Multituberculata, and should be adopted. There is no question as to the priority of definition of the Allotheria; let us consider the character and applicability of the definition. A fundamental objection to this term, as well as to Pantotheria, is that Gill and Huxley had previously employed the termination *theria* for the three great subclasses of Mammalia (represented to-day by the Monotremata, Marsupialia, and Placentalia); the termination had thus acquired a distinctive and useful subclass rank. Even, therefore, if Allotheria had been properly defined, it could not well be adopted for a group which

[16] I have found a number of errors in my writings upon the Mesozoic mammals which are not in the author's list. I have also taken great care in every instance to publish corrections of them as soon as discovered.

constitutes at most an order included in the Prototheria or Metatheria. We may consider it fortunate, therefore, that the term was not given a distinctive ordinal definition, but one which, upon the author's own statement,[17] failed to separate it from the Marsupialia,—viz: 1. Teeth much below normal number; 2. canine teeth wanting; 3. Premolar and molar teeth specialized; 4. Angle of lower jaw distinctly inflected; 5. Mylohyoid groove wanting. The best criterion of the definition and of the inutility of this term Allotheria is the fact that it has been rejected by every subsequent writer.

The third and fourth points advanced by the author in reply are extremely comprehensive, and, if they can be substantiated, will to some extent invalidate my criticism. It will be observed, however, that both points are advanced very cautiously.

3. He states that "no true Plagiaulacidæ are known with three rows of tubercles upon the upper molars." This is equivalent to saying that we have no positive evidence that the upper molars have three rows; it is not stated by the author that any upper Plagiaulax molar is *known* with two rows. I may recall the fact that in my review I called attention to this lack of positive evidence, and enumerated the strong cases of indirect evidence to the contrary which we find, first, in the association of loose molars of three rows of tubercles with the lower jaws of Neoplagiaulax, not to speak of Cimolomys; second, in the three rows of the maxillary molars of Polymastodon, a genus very closely related to Plagiaulax; third, in the analogy of Tritylodon. I consider this indirect evidence so strong that it is very unlikely to be refuted by subsequent discovery, and believe that the author will never be able to substantiate this first statement.

4. Finally, he states that "no Allotheria (Multituberculata) are known with certainty to have three rows of tubercles in the lower molars." In support of this cautious assertion, the author first refers to the type of Stereognathus as probably a portion of a maxilla; this probability we have already shown rests upon no stated evidence whatever. He observes, secondly, that "there is now conclusive evidence that the Cretaceous molar teeth with

[17] *Amer. Jour. of Science*, 1880, p. 239.

three rows of crescentic tubercles belong to the upper series, as I have described them."

In reply, I may say that undoubtedly some crescentic tubercular teeth, in three rows, will be found in the upper jaws of some Cretaceous species, if for no other reason, because the ancestors of Polymastodon will be found in the Cretaceous, and these ancestors will exhibit three rows of subcrescentic tubercles in the upper molars, since such is the character of the large Polymastodon upper molars.

It remains for the author to show specifically that the types of Selenacodon and Tripriodon are maxillary teeth. I should myself have considered them as such but for the fact that the type of *Dipriodon robustus*, with two rows of tubercles, was described as a maxillary tooth, and figured with a supposed fragment of the zygomatic arch attached to the alveolar border. If this tooth proves to be mandibular, and the molars with three rows of crescents are shown to be maxillary, the author will have substantiated his second statement; but such proof will not help him out of his synonomic dilemma, for it will still appear that he has founded three families, five genera, and as many species upon different teeth belonging to the same dental series, and all synonyms of Meniscoëssus.

Enough has been said to make it clear that, whatever evidence the author may hold in reserve for his future memoir, this "Note on Mesozoic Mammalia" contains no positive evidence in defence of his "Cretaceous Mammalia."

In closing, I may quote a concluding paragraph in the author's reply: "No one who has earnest work to do can afford to spend time in the ungracious task of pointing out errors in the work of others." I have always been of the same opinion that criticism is an ungracious and thankless task. In this case I deferred my "Review" for nearly two years, and endeavored to avoid it entirely by sending to the author all my main points of doubt in regard to his paper, and asking him to revise it. After waiting for the author to avail himself of this opportunity, I came to the conclusion that the "Cretaceous Mammals," sent out with the authority of the distinguished author's name, and under the

auspices of our National Survey, might spread abroad a score of synonyms which, finding their way into literature, it would require years to eliminate. I am happy to see, however, that all recent writers, guided by their own critical faculty, and in some measure perhaps by my "Review," have taken from the two papers on "Discovery of Cretaceous Mammals" only what they actually contain,—namely, valuable and interesting additional characters of two or three multituberculate genera, already partly known, besides the discovery of two small trituberculates, and have not recognized the four orders, eight families, sixteen new genera, and twenty-seven new species constituted by the author.

HEMLOCK AND PARSLEY.

BY W. W. BAILEY.

THE study of the order Umbelliferæ presents peculiar difficulties to the beginner, for the flowers are uniformly small, and strikingly similar throughout the large and very natural group. The family distinctions or features are quite pronounced and unmistakable, and it is the determination of the genera which presents obstacles, — serious, indeed, but not insurmountable. "By their fruits shall ye know them."

The Umbelliferæ, as we see them here, are herbaceous, with hollow, often striated stems, usually more or less divided leaves, and no stipules. Occasionally we meet a genus, like Eryngium or Hydrocotyle, with leaves merely toothed or lobed. The petioles are expanded into sheaths; hence the leaves wither on the stem. The flowers are usually arranged in simple or compound umbels, and the main and subordinate clusters may or may not be provided with involucres and involucels. To this mode of arrangement there are exceptions. In marsh-penny-wort (Hydrocotyle) the umbels are in the axils of the leaves, and scarcely noticeable; in Eryngium and Sanicula they are in heads. The calyx is coherent with the two-celled ovary, and the border is either obsolete or much reduced. There are five petals inserted on the ovary, and external to a fleshy disk. Each petal has its tip inflexed, giving it an obcordate appearance. The common colors of the corolla are white, yellow, or some shade of blue. Alternating with the petals, and inserted with them, are the five stamens.

The fruit, upon which so much stress is laid in the study of the family, is compound, of two similar parts or carpels, each of which contains a seed. In ripening, the parts separate, and hang divergent from a hair-like prolongation of the receptacle known as the gynophore. Each half fruit (mericarp) is tipped by a persistent style, and marked by vertical ribs, between or under which lie, in many genera, the oil-tubes or vittæ. These are channels

containing aromatic and volatile oil. In examination the botanist makes delicate cross-sections of these fruits under a dissecting microscope, and by the shape of the fruit and seed within, and by the number and position of the ribs and oil-tubes, is able to locate the genus. It of course requires skill and experience to do this, but any commonly intelligent class can learn the process. It goes without saying, and as a corollary to what has already been stated, that these plants should always be collected in full fruit; the flowers are comparatively unimportant. Any botanist would be justified in declining to name one of the family not in fruit. An attempt would often be mere guesswork.

In this family is found the poison hemlock (Conium) used by the ancient Greeks for the elimination of politicians. It is a powerful poison. The whole plant has a curious mousy odor. It is of European origin. Our water hemlock is equally poisonous, and much more common. It is the *Cicuta maculata* of the swamps,—a tall, coarse plant which has given rise to many sad accidents. *Æthusa cynapium*, another poisonous plant, known as "fool's parsley," is not uncommon, and certainly looks much like parsley. This only goes to show how difficult it is for any but the trained botanist to detect differences in this group of plants. Side by side may be growing two specimens, to the ordinary eye precisely alike, yet the one will be innocent and the other poisonous.

The drug assafœtida is a product of this order. All the plants appear to "form three different principles: the first, a watery acid matter; the second, a gum-resinous, milky substance; and the third, an aromatic, oily secretion. When the first of these predominates they are poisonous; the second in excess converts them into stimulants; the absence of the two renders them useful as esculents; the third causes them to be pleasant condiments." So that besides the noxious plants there is a long range of useful vegetables, as parsnips, parsley, carrots, fennel, dill, anise, caraway, cummin, coriander, and celery. The last, in its wild state, is said to be pernicious, but etiolation changes the products and renders them harmless. The flowers of all are too minute to be individually pretty, but every one knows how charming are the

umbels of our wild carrot, resembling as they do the choicest old lace. Frequently the carrot has one central maroon-colored floret.

Though most of the plants are herbs, Dr. Welwitsch found in Africa a tree-like one, with a stem one to two feet thick, much prized by the natives for its medicinal properties, and also valuable for its timber. In Kamschatka also they assume a sub-arboreous type, as well as on the steppes of Afghanistan.

As mistakes often occur by confounding the roots of Umbelliferæ with those of horse-radish or other esculents, it is well, when in doubt, to send the plants, *always in fruit*, if possible, for identification. None of them are poisonous to the touch,—at least to ordinary people. Cases of rather doubtful authenticity are reported from time to time of injury from the handling of wild carrot. We have always suspected the proximity of poison ivy; still, it is unwise to dogmatize on such matters. Some people cannot eat strawberries—more's the pity!—while the rest of us get along with them very happily. Lately the *Primula obconica* has acquired an evil reputation as an irritant, so there is no telling what may not happen with certain constitutions.

Difficult as is the study of Umbelliferæ, it becomes fascinating on acquaintance. To hunt up a plant and name it by so scientific a process brings to the student a sufficient reward.

Providence, Rhode Island, July 2d, 1891.

THE PROBLEM OF THE SOARING BIRDS.

BY I. LANCASTER.

SAYS Darwin, in the naturalist's voyage around the world: "When the condors are wheeling in a flock round and round any spot, their flight is beautiful. Except when rising from the ground, I do not recollect ever having seen one of these birds flap its wings. Near Lima I watched several for nearly half an hour, without once taking off my eyes: they moved in large curves, sweeping in circles, descending and ascending without giving a single flap. As they glided close over my head, I intently watched from an oblique position the outlines of the separate and great terminal feathers of each wing; and these separate feathers, if there had been the least vibratory movement, would have appeared as if blended together; but they were seen distinct against the blue sky. The head and neck were moved frequently, and apparently with force; and the extended wings seemed to form the fulcrum on which the movements of the neck, body, and tail acted. If the bird wished to descend, the wings were for a moment collapsed; and when again expanded with an altered inclination, the momentum gained by the rapid descent seemed to urge the bird upwards with the even and steady movement of a paper kite. In the case of any bird *soaring*, its motion must be sufficiently rapid so that the action of the inclined surface of its body on the atmosphere may counterbalance its gravity. The force to keep up the momentum of a body moving in a horizontal plane in the air (in which there is so little friction) cannot be great, and this force is all that is wanted. The movement of the neck and body of the condor, we must suppose, is sufficient for this. However this may be, it is truly wonderful and beautiful to see so great a bird, hour after hour, without any apparent exertion, wheeling and gliding over mountain and river."

The above remarkable words for many years have served to sustain my interest in the endeavor to substitute the actual for his provisional solution of soaring flight.

I presented in this magazine certain papers of a suggestive character, the results of much observation and experiment, in the hope of directing attention to curious facts, not recognized, bearing upon this subject. Three years ago I moved to my present location, Egeria, Colorado, where, during the past eighteen months, I have done the principal work necessary for this undertaking.

I consider myself successful in obtaining important results. In this paper I propose dealing with an experiment so obviously simple, as it now stands, that I am dazed to think of the labor that has been expended upon it. This device, in wind, really exhibits the entire soaring case in a very concise way, so far as its fundamental principles are concerned; but I give briefly the same experiment in calm air, as supplementary. The first form of the experiment can be tried by any one with a fair amount of constructive ability, and it cuts an awful chasm through the teachings of the mechanical schools on atmospheric resistance, and unerringly points to practical air navigation.

A rough-board platform, about twelve feet square, with a post and cross-beam fixed upon it, the whole being capable of horizontal rotation to present the plane to the wind from whatever direction it might blow, is the framework.

The plane should be a flat pyramid,—5x5 feet square is a good size on the base, and two inches high. The edges of such shape, being sharp, give the effect of a true plane without thickness. The whole should be finished smooth, similar to the top of an ordinary piano. Perfectly seasoned white pine or red cedar is an excellent material to use, glueing thin strips together in the rough, and dressing to shape.

To one edge of the plane two fine steel wires are fastened, which are suspended from two ordinary spring balances, fixed by their rings to the cross-beam of the platform. When thus suspended the plane will sway freely too and fro, like a child's swing, the flat side being in a vertical position when at rest.

To the apex at the back of the plane is fixed another wire provided with another spring balance, the ring of which is fastened to the rear part of the cross-beam, when the plane is drawn back-

ward to any desired angle of obliquity with the horizon. This wire must in all cases be kept perpendicular, and the other two parallel to the plane.

It is obvious that the plane can be pulled backwards through all degrees of the quadrant, and suspended at any position by the wires and balances in a state of rest. While the plane hangs vertically all its weight will be on the parallel wires, half on each. As it is pulled to the rear less and less weight will be on the parallel wires, and more and more on the normal wire, until at a horizontal position, 90° from the starting point, all weight will be on the normal wire, and none on the parallel.

If we assume the soaring inclination of a bird to be 5° from horizontal, and pull the plane back to that angle, the normal scale will mark twenty-seven pounds and the parallel one pound each, small fractions of weight being neglected. If an angle of 45° be used, the normal scale will mark about nineteen pounds and the two others 9½ pounds each.

FIG. 1.

To operate this device, a time of calm air is chosen, and the plane is pulled back to any desired angle, say 45°, the normal scale fastened in position, and the weight indications of each scale noted. When wind arises, rotate the platform to face it, so that the plane will stand squarely across its direction. This is not done to expose the plane to any particular direction of wind, but to get all the resistance possible out of it. Then observe the scales to see what effect wind has on the normal and parallel gravity stresses. Any perceptible breeze will be shown by the normal scale, which will indicate less and less weight as wind increases. If the air moves with sufficient force, all weight will

be taken from the normal scale, the wire may be cut, and the plane will rest on the air pressure beneath its surface, in which event pressure is substituted for tension on the normal line, otherwise the stress being unchanged, either in direction or magnitude.

If the plane be suspended at the soaring angle of 5°, and wind is strong enough to lift it to that angle, all tension will be taken from the normal scale, the plane resting on air pressure as at the 45° inclination, and so on through all degrees of the arc.

During all this time the parallel scales indicate the weight recorded by them at the various angles in calm air, so that they are not influenced by wind in the least degree, however great its force might be.

If at any angle of obliquity the plane be held by a wire from each corner, perpendicular to it, and fastened to the platform, no force of wind will vary the parallel scales, the tension of the four wires holding the entire wind resistance, however great, In short, there is no way to get wind to affect the parallel scales. If variation in them occurs, it shows at once some error in the experiment, either the wires being out of adjustment, or some warpage of plane or other disturbance having taken place.

The rotating arms for trying this experiment in calm air are 60 feet radius, their ends describing a circle of 120 feet diameter. The plane is hung under the arm, and all the conditions of the other experiment complied with. No change could be noted in the results, excepting that motions of plane were made with greater smoothness, on account of greater steadiness in artificial wind. By no means could air resistance be brought to bear on the parallel component of gravity. It acted the same in wind as in calm air, undisturbed by atmospheric resistance.

Before anything is inferred from these experiments it will be best to state the facts of the case. So completely subversive of the usual conceptions of the action of air on surfaces is this trial that it seems imperative to get the facts right before attempting to range them in the order of mechanical sequence.

For convenience, I will number a few of the conspicuous ones.

1. The scales, being marked in half pounds, do not register

small fractions of force, so that if there be friction down the plane caused by slipping air, such resistance would act on the parallel scales, but, being of small magnitude, could not be read from the index.

2. Neither the cross-arm in wind, nor the rotating arm in calm air, holds the plane against the air. They simply neutralize the parallel factor of gravity, and prevent its acting upon the plane.

3. The normal gravity component does work upon the air by disturbing it in various ways, such as changing its motion, and condensing it, and it is the only fraction of weight that meets with atmospheric resistance, if we neglect the unknown elements of friction.

4. The experiments are in obedience to the law of normal resistance of fluids to surfaces compressing them.

5. There is no resistance to the plane on the line of motion of either plane or air. Even the hypothetical friction is not on such line.

6. Wind or calm air produce identical results; the only requirement being that air and plane shall meet, motion of either, related to the earth, being indifferent.

The consequences flowing from the above are many and important, and in the briefest manner I will notice those which seem to demand the earnest attention of every student of nature, however abhorrent they may be to text-book devotees.

It is obvious that the wires of the experiment, when the plane is adjusted to any obliquity in calm air, resolves weight in the same way that an ordinary inclined plane would if the latter were rough, the stresses being of the nature of pressure instead of tension.

Then we have in the mathematical formula of inclined planes without friction what also applies to oblique atmospheric resistance. For when the plane of the experiment, held by the wires, is submitted to air pressure, the shape, size, and obliquity of that plane determines the shape, size, and obliquity of the plane of air pressure beneath it, which plane being destitute of friction the component along it must be neutralized, as is done by the paral-

lel wires, to preserve stability; otherwise, the experimental plane would slip down the air plane.

It follows that atmospheric resistance is a purely static feature in all cases; as completely so as a rigid inclined plane, resolving all forces impinging upon it. This becomes still more evident when we see that it is the experimental plane that determines the direction of air resistance, and not the direction in which plane and air meet. If air moved vertically upwards, or from the rear, or from any other direction, against the lower surface, the direction of resistance would be unchanged, and the experiment equally effective. It was not to employ horizontal wind that the experiment was arranged, but to parallel the case with a soaring bird, the working force of which is gravity.

Magnitude of resistance would vary, with the same wind, as its direction approached or receded from the normal line, but, as will be shown farther on, magnitude is unimportant, direction being the vital matter.

If the experimental plane were supposed to be of the same specific gravity as the air it displaces, and some other force used to hold the plane against, or drive it upon air, the same result would follow. This force would be resolved, as gravity is resolved, by the air plane.

The enormous error of the mechanical schools in estimating resistance of air to oblique surfaces is now conspicuous. This error has the sanction of the great name of Newton, and stands squarely across the pathway leading to artificial air navigation, setting up scarecrows along every avenue to success.

But it is consoling to know that no man, not even a Newton, can diagram a force into nature that was not there before the diagram was made.

The entire nature of air resistance is misconceived. The case is a curious one of *bouleversement.* The activities are turned upside down. Instead of the surface resolving the resistance, it is the resistance that resolves whatever forces drive the surface upon it. I have examined over a hundred text-books of mechanical teaching, and have found no exception to the prevalence of this error. They all get resistance on the line in which

plane and air meet. They do not all go about it in the same way, but in the end that is where they locate it. So far as I can determine, the error is on the increase, the later authorities being more emphatically astray than the earlier ones. The last utterance that I have seen is contained in a lecture delivered to the students of Sibly College, Cornell University, in May, 1890, by O. Chanute, C.E. On pages 28 and 29 of the published paper the error comes out glaringly. In speaking of M. Drzewieki's work on Duchemin's formula, he says: "Next he calculates the horizontal components of the normal pressure, this being the resistance to the advancement of the plane." His entire exposition is saturated with the blunder, for no horizontal resistance exists. Mr. S. P. Langley, in his recent submission of experiments and suggestions in aërodynamics, entertains the same delusion in its full force, otherwise he seems notably free from a priori bias.

But my thanks are due to some unknown friend who has just sent me a copy of *Science*, dated May 1st, 1891, in which I find marked a letter to the editor on "Flying Machines," by H. A. Hazen. The writer is wholly unknown to me; but I infer that he is neither a "crank" nor a dabbler in science, but a well-grounded expert of wholly intelligent attainments. Without presuming to defend Mr. Langley in the remotest manner, I am justified in holding that Hazen's statements are accepted by the mechanical schools, and I herewith call attention to the utterly erroneous nature of such teaching by contrast to the following facts.

1. A bird does support itself in soaring flight by going fast, and in no other way.

2. There is no evidence that a bird in the act of soaring ever remained stationary in still air for an instant; and whether air is hurled against bird or bird against air, the result is the same; meeting in opposition of bird and air is required by the very nature of soaring flight.

3. "Solution of the problem" is unchanged by "great velocity."

4. As parallel motion on the upward slant is what gives "velocity" to the bird, and as atmospheric friction is the only

resistance to such translation, speed of flight is determined by a force which may, for all practical purposes, be left wholly out of the account.

5. "As a matter of fact," the statement that "it would be much easier to go slow than fast" is a burlesque.

6. Resistance of air at one mile or one hundred miles per hour is practically the same.

So far as I can see, the paraphrased arguments of LeConte do not touch the problem of soaring flight in any way, but all the errors of the mechanical schools are evidently adopted by him.

But here is by no means the end of this catena of difficulty.

There are three ways, shown in the subjoined diagram, in which a surface and air can meet.

1. Parallel to the surface.
2. Obliquely to the surface.
3. Normal to the surface.

The first meets with frictional resistance.

The second with both friction and pressure.

The third with pressure resistance only.

The direction of the first and third resistance is in the line on which plane and air meet, there being a single stress on that line. The directions of the second resistances are both parallel, and normal to the plane, there being two stresses perpendicular to each other, and no stress on the line in which plane and air meet.

It directly follows that direction of motion of the second case is a resultant, composed of two velocities, one on the line of frictional, the other on the line of pressure resistance, the plane being a body subject to two forces, neither of which are in the line of motion.

No single force, however great, could drive a plane obliquely through air. There must of necessity be not less than two stresses, derived either from the application of two forces, or the resolution into two components of one.

I need go no further in this direction, but will add a few words regarding my work during the past year and a half, in the way of direct flight.

It is sufficiently obvious that a force of about two pounds' constant pressure applied to the plane, either in wind or calm air, in the above experiments, to take the place of the cross-arm and rotating arm, neutralizing the parallel component of gravity, would, if obtained from the constant flow of twenty-seven pounds pressure escaping from beneath the surface, produce a true soaring plane in calm and wind.

A smooth plane has no tendency to throw two pounds of this twenty-seven pounds (over 90°) against the obstructive component.

The true problem of flight consists in so manipulating the surface as to perform this function. I have hitherto taken the wing of a soaring bird as a model, and have had unexpected and most gratifying success.

I can produce true soaring flight in natural wind with a plane exceeding two pounds to a square foot of surface whenever I wish to do so and can obtain wind strong enough for the purpose. During the above-mentioned period of time I have made about fifty planes of various shapes and sizes, and from 25 to 400 pounds in weight. These planes are not set free in wind, but used as in the experimental cases above, with rigid rods in place of the parallel wires. These rods run in large rings and have a cross head at their outer ends, allowing the plane to run to the front until its edge rests against the rings. In the best trials the parallel component is neutralized at 10° from horizontal, far exceeding my expectations derived from observations of the birds; their angle of obliquity being rarely over 5°.

On a few occasions these planes accidentally escaped me in time of highest wind, and were ruined at once for all purposes excepting fire-wood; in each case being a loss of two or three months' work, and playing havoc with my finances. One that I

valued particularly plunged to the front in a violent blast of wind with force sufficient to tear out the rings. It rose into the air gradually higher and higher, until an elevation of at least 3,000 feet was attained, when some part of the device giving away it lost equilibrium, and plunged through the air, striking the earth about two-and-a-half miles from the starting point, and 1000 feet higher than that locality. Another mile would have carried it to the summit of the Flat Top Mountains. It was in the air about three hours, and I walked beneath it during its flight. Its course was directly against the highest wind I have experienced during my residence here. At times it did not progress, but went higher. It weighed one hundred and ten pounds, and had been well balanced for experimenting on surface manipulation. There was no lesson taught in this flight, the birds having been doing the same thing for a long time. It was an interesting spectacle to look at; so is a large bird in the same act. I presume Mr. Darwin's provisional solution would apply to this plane as well as to the condors, but I am trying to explain the actual mechanical activity of both.

The best effects produced were with a plane of 400 pounds weight and eighty square feet of surface. In a wind that would be rightly termed a gale, arising about midnight, this plane was thrown to about 7° from horizontal. It ran to the front against the rings at 10°, where the entire parallel component was neutralized, and at 7° it hugged the rings with a force that required a backward pull of fifteen pounds to detach it.

This plane would make a splendid air navigator, and I would have no hesitation in trusting myself to it when steering, equilibrium, and alighting or stopping items had been worked out. I mean to say that it would navigate wind. I am now just entering on a course of experiments in calm air.

The front ledge is an important factor in steadiness, but the rear curve I have entirely abandoned, surface manipulation being depended upon to produce the parallel thrust. My judgment is that I have succeeded in separating the mechanical devices of a bird's wing which produce flight from those serving other life purposes of the animal. It has been an arduous task. At the

close of experimenting in this line I hit upon a method not exhibited by the birds, which promises best results, but which waits further developments.

Let us suppose a soaring bird of twenty-seven pounds weight, with stretched wings on an angle of 5° from horizontal, translating itself in calm air, or stationary when related to the earth in wind. Suppose, further, the initial impulse to have been made so that its motion is uniform.

Then its weight is resolved into a normal and parallel component by the air plane of pressure beneath it. The normal component does work by driving the air out of place, thereby changing its motion. The parallel component acts upon the bird, tending to drive it on the downward slant, backwards. The air in being condensed reacts in all directions, and reaction towards the front being resisted by the surface manipulations, the bird is driven against friction and the small component, on the upward slant, and, completing the diagram of velocities, the result is horizontal flight.

FIG. 2.

If the parallel factor be supposed out of the way by neutralization arising from surface manipulation, and the air to be calm, then the normal factor would drive the plane against air pressure from *a* to *b* in any given unit of time, say one second.

Simultaneously with this motion a small excess of parallel force over that required to balance the parallel factor—say one-half pound constant pressure—would drive the plane from *a* to *c*, against friction, in which case motion would occur on *a d*, the resulting line of horizontal flight.

I have said that direction, and not magnitude of force, was the vital point in the soaring activity. This fact must now be quite

plain. Magnitude of force depends on weight of plane, and on that alone. The gravity stress existing between the plane and the earth's center is the only conceivable force in action, and air will meet any demand made upon it by the normal factor of this force, against which it is a reaction. This factor is determined by the angle of obliquity of plane in each case, and the work done upon the air goes on independently of the parallel motion of the plane on the upward slant, which alone gives a soaring movement. It is obvious that whether wind should move from the zenith downwards, or from the earth vertically upwards, or horizontally, or from any angle of obliquity whatever, the soaring activity is not affected. That goes on as it does in calm air. Motion of plane as related to the earth would of course vary with wind, if inclination remained the same, but the soaring action, considered by itself, is absolutely independent of wind,—meaning by that word motion of air as related to the earth.

I had prepared specimen feathers from the wings of various birds during the past twenty-five years, intending to prepare a paper for publication on the subject of surface manipulation, which I hold to be the most curious and interesting feature about a bird, yet almost paralleled by the wonder that it has hitherto escaped notice. But we have a rat, if it be a rat, that is endowed with a peculiar moral nature. With it, exchange is no robbery. A nest of them inhabited the building in which I kept my specimens, and they bartered what I laid claim to for an indescribable lot of miscellaneous rubbish of no earthly use to me. Their views and mine were very different. They had cut the feathers into small bits and mixed them with dirt, and stowed them away in all sorts of holes and corners. I shut a cat in the room for some days, which caused the rodents to vanish, but my feathers were ruined.

If you will take a primary wing feather of any large bird,—say a wild goose, or especially a frigate bird,—and cut squarely across the ribs, and examine the divided part endwise with a magnifying glass of low power, you will see nature's plan of surface manipulation to get the forward thrust against the small component of weight and air friction. Cut on *b c*, and examine

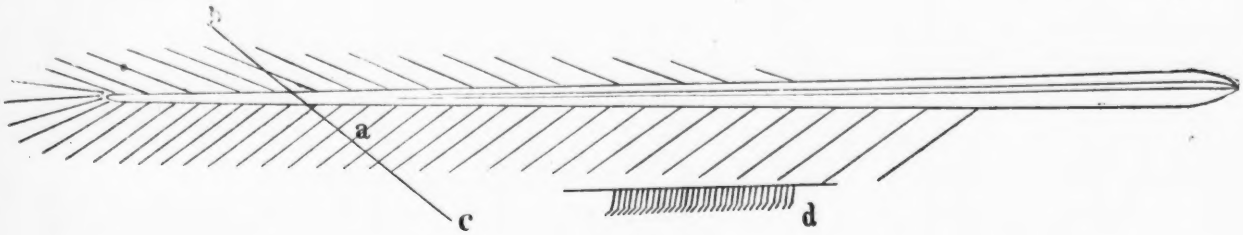

FIG. 3.

the cut ends of the ribs along *a*. You will see something like *d*. If we suppose an unpressed small portion of air to be represented by a small circle o, and the same air when pressed, by a flat disk ⬭, and then mesh this disk with the sort of surface made by the manipulation, the nature of the front thrust will be made thinkable. I enlarge it below. That part of the air particle to

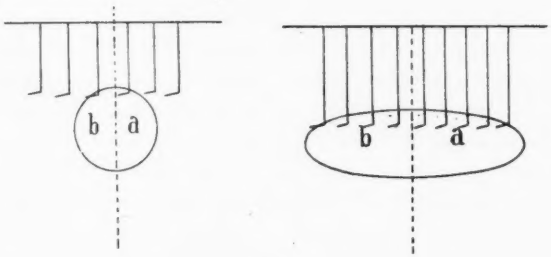

FIG 4.

the rear of the dotted line as *b* slips easily over the ribs, while the front part, *a*, engages with them and tends to carry them to the front. I have worked out all this artificially with excellent results, and can give you a paper upon it if a large part of the actual size and shape of the ribs in different birds is left to memory, as the rats have destroyed my samples.

THE HISTORY OF GARDEN VEGETABLES.

BY E. L. STURTEVANT.

(Continued from page 706, Vol. XXV., 1891.)

Lycopersicum humboldtii Dun.

This is very like the preceding, but the racemes of the flowers smaller, the calycine segments being never the length of the corolla, and the berries one-half smaller, red, and, when cultivated, not less angular than those of *L. esculentum.*[1] It was noticed by Humboldt[2] as under cultivation at La Victoria, Neuva Valencia, and everywhere in the valleys of Aragua, in South America, and is described by Kunth[3] in 1823, and by Willdenow, about 1806, from plants in the Berlin garden from seeds received from Humboldt. The fruit, although small, has a fine flavor. I suspect the Turban, Turk's Cap, or Turk's Turban of our seedsmen, a novelty of 1881, to be referable here, although this cultivated variety is probably a monstrous form.

Lycopersicum pyriforme Dun.

This, which is to be classed as one of the fancy varieties under cultivation, occurs with both yellow, red, and pale yellow or whitish fruit. It was described by Dunal in 1813, and in Persoon's synopsis in 1805.[4] It is mentioned in England in 1819, and both the colors in the United States by Salisbury[5] in 1848. It is liked by some for garnishing and pickling. The common names are pear-shaped and fig.

Lycopersicum pimpinellifolium Dun.

The currant tomato bears its red fruit, somewhat longer than a common currant, or as large as a very large currant, in two-

[1] Don. Gard. and Bot. Dict., IV., 443.
[2] Humboldt. Trav., Bohn. Ed., II., 20.
[3] Kunth. Syn., 1823, II., 157.
[4] Dunal. Solan., 110.
[5] Salisbury. Trans. N. Y. Ag. Soc., 1848, 371.

ranked racemes, which are frequently quite large and abundantly filled. It grows wild in Peru and Brazil, and is figured by Feuille[6] in 1725, but not as a cultivated plant, and is described by Linnæus[7] in 1763. The grape or cluster tomato is recorded in American gardens by Burr[8] in 1863, and as the red currant tomato by Vilmorin[9] in 1883 and 1885. It is an exceedingly vigorous and hardy variety, with delicate foliage, and fruits most abundantly. The berries make excellent pickles.

According to the test of cross-fertilization, few, if any, of the above are true species. Two only of the above named—the cherry and the currant tomato—do I find recorded in a truly wild condition. The tomato has, however, been under cultivation from a remote period by the Nahua and other Central American nations, and reached European and American culture, as all the evidence implies, in an improved condition. If there is any evidence that any of our so-called types arose spontaneously from the influences of culture, I have failed to note it. We may well ask, Why did not other forms appear during the interval from 1558 to 1623, when but one sort, and that figured as little able,vari received the notice of the early botanists?

The modern names of the *tomato*, or *love apple*, are in France, *tomate, pomme d'amour, pomme d'or, pomme du Perou;* in Germany, *tomate, liebesapfel;* in Flanders and Holland, *tomaat;* in Italy, *pomo d'oro;* in Spain and Portugal, *tomate;* in Norway, *kjoerlighedsaeble.*[10]

In Arabic, *bydingan toumaten;*[11] in Burma, *kha-yan-myae-phung;*[12] in Ceylon, *maha-rata-tamattie;*[13] in the Deccan, *wall-wangee;* in Egypt, *bydingan toumaten;* in Malaya, *tomatte;* in Tagalo, *tomates, camatis;*[12] in Tamil, *seemie-takalie-pullam;*[13]

[6] Feuille. Per., 1725, 37, t. 25.
[7] Linnæus. Sp., 1763, 265.
[8] Burr. Field and Gard. Veg., 1863, 646.
[9] Vilmorin. Les Pl. Pot., 554.
[10] Schubeler. Culturpf., 91.
[11] Delile. Fl. Æg. Ill.
[12] Pickering. Ch. Hist., 615.
[13] Birdwood. Veg. Prod. of Bomb., 173.

in Indian gardens, *goot-begoon, oou laeetee buengun ;*[14] in Mexico, *jomatl ;*[15] in Japan, *akanasu,* red egg-plant.[16]

TURNIP. *Brassica* sp.

Vilmorin in his "Les Plantes Potageres," 1883, classes all the *turnips* under *Brassica napus* L.; but the older authors referred them, more correctly as we think, to *Brassica napus* and *B. rapa.* Decandolle,[17] who makes this distinction, separates the first into three groups, based on color, the white, yellow, and black; the second into groups, comprising the white, yellow, black, red, and green. In the thirteenth century Albertus Magnus describes the *napus* as with a long root, which is eaten, and the *rapa* as having a spherical compressed bulb, and sometimes red in the stalk. The turnip is of ancient culture. Columella,[18] A. D. 42, says the *napus* and the *rapa* are both grown, and the latter the larger and greener for the use of man and beasts, especially in France; the former not having a swollen, but a slender, root. He also speaks of the *Mursian gongylis,* which may be the round turnip, as being especially fine. The distinction between the *napus* and the *rapa* was not always held, as Pliny[19] uses the word *napus* generically, and says that there are five kinds, the Corinthean, Cleonæum, Liothasium, Bœoticum, and the Green. The Corinthean, the largest, with an almost bare root, grows on the surface, and not, as do the rest, under the soil. The Liothasium, also called Thracium, is the hardiest. The Bœoticum is sweet, of a notable roundness, and not very long as is the Cleonæum. At Rome the Amiternian is in most esteem, next the Nursinian, and third our own kind (the green?). In another place, under *rapa* he mentions two kinds, the one broad-bottomed (flat?), the other globular, and the most esteemed those of Nursia. The *napus* of Amiterninum, of a nature quite similar to the *rapa,* succeeds best in a cool place. He mentions that the *rapa* sometimes attain a weight of

[14] Speede. Ind. Handb. of Gard., 176.
[15] Heller. U. S. Pat. Of. Rept., 1874, 411.
[16] K. Tamari.
[17] Decandolle. Mem., 1821, 26, 30.
[18] Columella. Lib., II., c. 10, etc.; X., c. 421.
[19] Pliny. Lib., XIX., c. 25; Lib., XVIII., c. 34, 35.

forty pounds. This weight has, however, been exceeded in modern times. Matthiolus,[20] in 1558, had heard of turnips that weighed a hundred pounds, and speaks of having seen long and purple sorts that weighed thirty pounds. Amatus Lusitanus,[21] in 1524, speaks of turnips weighing fifty and sixty pounds. In England, in 1792, Martyn[22] says the greatest weight that he is acquainted with is thirty-six pounds. In California, about 1850, a turnip is recorded of one hundred pounds weight.[23]

Brassica napus esculenta DC.[17]

This differs from the *Brassica rapa oblonga* by its smooth and glaucous leaves. It surpasses other turnips by the sweetness of its flavor, and furnishes white, yellow, and black varieties. It is known as the Navet or French turnip.[17] It was apparently the *napa* of Columella.[18] It was certainly known to the early botanists, yet its synonymy is difficult to be traced from the figures. I think, however, the following are correct:

Napus. Trag., 1552, 730; Matth., 1554, 240; Pin., 1561, 144; Cam. Epit., 1586, 222; Dod., 1616, 674; Fischer, 1646.

Bunias sive napus. Lob. ic., 1591, I., 200.

Bunias silvestris lobelii. Ger., 1597, 181.

Napi. Cast. Dur., 1617, 304.

Bunias. Bodæus, 1644, 773.

Napus dulcis. Blackw., 1765, t. 410.

Navet petit de Berlin. Vilm., 1883, 360.

Teltow turnip. Vilm., 1885, 580.

The *navews* are mentioned as under cultivation in England by Worlidge,[24] in 1683, as the *French turnip* by Wheeler,[25] in 1763, and Miller's Dictionary, 1807. Gasparin[26] says the *navet de Berlin*, which often acquires a great size, is much grown in

[20] Matthiolus. Com., 1558, 240.
[21] Amatus Lusitanus. In Diosc., 1554, 247.
[22] Martyn. Fl. Rustica, 1792.
[23] A. Williams. U. S. Pat. Of. Rept., 1851, 4.
[24] Syst. Hort. By J. W. Gent, 1683, 181.
[25] Wheeler. Bot. and Gard. Dict., 1763, 81.
[26] Gasparin. Cours de Agr., IV., 116.

Alsace and in Germany. In China, according to Bretschneider,[27] it was known in the fifth century.

Brassica rapa depressa DC.

This has a large root expanding under the origin of the stem into a thick, round, fleshy tuber, flattened at the top and bottom. It has white, yellow, black, red or purple, and green varieties. It seems to have been known from ancient times, and is described and figured in the earlier botanies.

A. Flattened both above and below.

Rapum. Matth., 1554, 240; Cam. Epit., 1586, 218.
Rapum sive rapa. Pin., 1561, 143.
Rapa. Cast. Dur., 1617, 386.
Navet turnip. Vilm., 1883, 583.

B. Flattened, but pointed below.

Orbiculatum seu turbinatum rapum. Lob. ic., 1791, I., 197.
Rapum. Porta, Phytognom, 1591, 120.
Rapum vulgare. Dod., 1616, 673.
Rave d'Auvergne tardive. Vilm., 1883, 369.

C. Globular.

Rapum. Trag., 1552, 728.
Rapa, La Rave. Tourn., 1719, 113.
Navet jaune d'Hollande. Vilm., 1883, 370.
Yellow Dutch. Vilm., 1885, 588.

Brassica rapa oblonga DC.

This race differs from the preceding in having a long and oblong tuber tapering to the radicle. It seems an ancient form,—perhaps the Cleonæum of Pliny.

Vulgare rapum alterum. Trag., 1532, 729.
Rapum longum. Cam. Epit., 1586, 219.
Rapum tereti, rotunda, oblongaque radici. Lob. ic., 1591, I., 197.
Rapum oblongius. Dod., 1616, 673.
Rapum sativum rotundum and oblongum. J. Bauh., 1651, II., 838.

[27] Bretschneider. Bot. Sin., 78.

Rapa, La Rave. Tourn., 1719, 113.
Navet de Briollay. Vilm., 1883, 372.
Briollay turnip. Vilm., 1885, 591.

This representation by no means embraces all the turnips now known, as it deals with form only, and not with color and habits. In 1828 thirteen kinds were in Thorburn's American seed catalogue, and in 1887 thirty-three kinds. In France, twelve kinds were named by Pirolle in 1824, and by Petit in 1826. In 1887 Vilmorin's wholesale seed-list enumerates thirty-one kinds.

The turnip is believed to have reached England from Holland in 1550,[28] but before this it had reached the New World. In 1540 Cartier[29] sowed turnip seed at the present Montreal, in Canada. In 1609[30] turnips are mentioned in Virginia, as also in 1649;[31] they are mentioned as cultivated in Massachusetts in 1629.[32] In Peru they are said by Acosta,[33] in 1604, to have increased so abundantly as to become a nuisance in the planting of grain.

The *turnip* is called in France, *navet, gros navet, grosse rave, naveau, navet turnips, rabiole, rabioule, rave plate, tornep, turneps, turnip;* in Germany, *herbst-rube, stoppel-rube;* in Flanders and Holland, *raap;* in Denmark, *roe;* in Italy, *navone, rapa;* in Spain and Portugal, *nabo;*[34] in Arabic, *lift,*[35] *luft;* in Bengali, *shalgram;* in Persia, *shalgram;* in Sindh, *gokhru;*[36] in Japan, *busei, aona* (the round form).[37]

[28] Booth. Treas. of Bot.
[29] Cartier. Third Voy., Pinkerton Voy., XII., 667.
[30] A True Decl. of Va., 1610, 13.
[31] A Perfect Desc. of Va., 1649, 4.
[32] Higginson. Mass. Hist. Soc. Col., 1st Ser., I., 118.
[33] Acosta. Hist., 1604, 261.
[34] Vilmorin. Les Pl. Pot., 1883, 357.
[35] Delile. Fl. Æg. Ill.
[36] Birdwood. Veg. Prod. of Bomb., 137.
[37] Thunberg. Jap., 375.

EDITORIAL.

EDITORS, E. D. COPE AND J. S. KINGSLEY.

THE fortieth meeting of the American Association for the Advancement of Science was held at Washington, between August 17th and 25th, inclusive. It was a successful and enjoyable occasion, and the membership present exceeded 800. Representatives from almost all parts of the country, including the Pacific coast, were present. The papers read were generally of a high order of merit. A noticeable feature was the large number of new specialists who appeared, especially in the Biological and Geological Sections, an indication of increased activity in these fields, due in the main to the good work which has been going on in our universities. The overflow of entomological papers was large enough to give the Entomological Club plenty to do, and a good many geological papers went over to the Geological Society of America, whose meetings overlapped those of the American Association. The address of the retiring president, Professor George L. Goodale, on the possibilities of the introduction and cultivation of plants, now only known in the wild state, which shall be of utility to man, was of wide interest. The addresses of the vice presidents were as various as usual. That of the president of the Geological Section was technical, while that of the president of the Biological Section was didactic. Professor Prescott made an excellent presiding officer. Excursions were not allowed to interfere with the work of the association, and they were the more enjoyable on that account. The closing meeting, on the evening of the 25th, was an occasion of much hilarity. Not the least important contribution to this feeling was the address of a youth who presented the association with an invitation to hold the meeting of 1893 at Chicago, during the exposition. In well-considered language the association was asked to contribute its mite to one of the vast aggregate of congresses to be held there, and thus take part in the rejuvenation of the human race which it was anticipated would date from that event. Rochester was selected as the place of meeting for 1892.

—THE American Society of Geologists met on August 24th and 25th, during the meeting of the American Association in the

same building (the Columbian University), in Washington. As the 24th was occupied by the association in excursions, the meeting on the 24th was devoted to papers by foreign members who had arrived with the intention of attending the International Congress. These were Steinmann and Rothpletz, of Germany; Harker and Gregory, of England; Schmidt, Krassnow, and Pavlow, of Russia; De Geer, of Sweden; and Stephanescu, of Roumania. Some of these will appear in future numbers of the NATURALIST. The most important papers contributed by American members were those of President T. C. Chamberlin, Mr. R. T. Hill, and Mr. W. J. McGee. The society was the recipient of papers suitable for the International Congress, after it was decided to restrict the work of the latter body to discussion of definite questions only. Professor Stevenson, of New York, presided over the sessions with his usual ability, and Mr. McGee made an admirable secretary.

—THE fifth meeting of the International Congress of Geologists met at Washington from August 26th to September 1st, inclusive The attendance from foreign countries was larger than was anticipated, although not so large as the list published by the reception committee would indicate. The foreign members were distributed as follows: Austria-Hungary, 2; Belgium, 4; Canada, 3; France, 3; Germany, 17; Great Britain, 6; Mexico, 3; Norway, 1; Roumania, 2; Russia, 4; Sweden, 4; Switzerland, 2; total, 51. Several members were accompanied by ladies; Great Britain sent 2; Roumania, 2; and Russia, 1; total, 56. With some names not classified above, the total number present was about 65. The language selected for the conduct of the affairs of the congress was English, although French has been hitherto universally employed. This was due to the fact that the members of the U. S. Geological Survey, who had assumed the management of the congress, could neither speak French nor understand it when spoken.

Professor Newberry, the acting president, was ill, and unable to attend, so that his place was occupied successively by the first vice president, Professor Jos. Le Conte, and Professors Von Zittel of Munich, and Gaudry of Paris. The honorary presidents were Professors J. D. Dana and James Hall. The second and third vice presidents were Maj. J. W. Powell and Mr. Raphael Pumpelly.

The discussions as arranged by the committee of management had reference, 1st, to the general principles of stratigraphic correlation; 2d, to the system of colors for geologic maps; and 3d, to the system of classifications of the Plistocene deposits. The necessity for discussion of the principles of correlation was not very apparent, since those principles are well known and generally adopted. The discussion, however, brought out some consideration of intercontinental and transcontinental applications of these principles, which were entirely appropriate to an international congress. Much of the discussion, however, bore on the question as stated, and was either inappropriate or local in its bearings. The discussion on the classification of the Plistocene was based on two systems, one offered by Mr. McGee and the other by President Chamberlin. Both are physical systems, and not historical, as was observed by various gentlemen who took part in the discussion. They were, however, of universal application, and many interesting facts were brought out, especially with reference to the existence or not of an interglacial warm climate on both hemispheres.

The congress was entertained by receptions at the Arlington Hotel, the U. S. National Museum, and the private houses of Messrs. Thomas Wilson and S. F. Emmons. Nearly all the foreign members took part in the excursion to the Rocky Mountains, Grand Canyon of the Colorado, and Yellowstone Park, which left Washington, September 2d, from which they are expected to return about September 25th. Before the meeting of the congress many of the members visited the paleontological collections at New Haven and Princeton, and during the congress, the collection of Prof. Cope at Philadelphia. Some interesting specimens from New Haven were exhibited in the U. S. National Museum.

The congress was overtaken at its opening by a spell of hot weather, which constituted a drawback to the enjoyment of some of the members. As an educator to the younger American geologists it was a successful occasion. We can point out some material defects of management which would not have occurred had the meeting been held in Philadelphia, as originally intended; but we postpone this for another occasion.

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RECENT LITERATURE.

Morris's Aryan Race.[1]—This work opens with a general description of the races of mankind, and then proceeds to discuss the origin of the Aryans, their migrations, and their history in general. The author adopts the view of Latham and others, that their original home was Southeastern Russia, and not the highlands of Western Asia. He summarizes the evidence, which is largely derived from the Sanscrit language. That the original home of the Aryans was not Southwestern Asia is indicated by the fact that that language contains no names for the lion and tiger; that it was not from Northwestern Asia is shown by the absence of a name for the camel. The supposition that Scandinavia was the land is shown to be incorrect by the fact that there is no word for the reindeer, and because the Aryans were a pastoral people, and not dwellers in forests.

His discussion of the ethnic origin of the Aryans is of course speculative. He supposes that the dark-colored tribes which now inhabit the more southern latitudes from India to Spain were derived by descent from the negro race, while the northern light-colored types were derived from the Mongolian. The derivation of the Aryans from the Mongolians at a remote and prehistoric period is among the possibilities, but that the darker forms have a negro origin is scarcely so. The origin of both must be traced to prehistoric—say neolithic—man, and it may be that the light (Xanthochroic) Aryans really represent the race in its pure form, and that the darker races (Melanochroi) represent either the primitive neolithic race, or are the product of hybridization between it and the Xanthochroi.

Mr. Morris describes briefly the western migrations of the light tribes, the Celts, Teutons, and Slavonians; and those of the darker Pelasgi, Hellenes, etc.; and later treats of the eastern extension to Persia and India, which established the Iranian and Hindoo populations. These supposed migrations are illustrated by the descriptions of the Gauls and Teutons left us by the Roman authors.

The chapters on the religious, philosophical, and poetic achievements of the Aryans will instruct the general reader, and the concise definitions of the intellectual and moral merits of the race form a scientific index of the line of human progress. The last chapter contains a brilliant forecast of the increasing greatness and power of this the latest and the best product of human evolution.

[1] The Aryan Race: Its Origin and Its Achievements. By Charles Morris. S. S. Griggs & Co., Chicago, 1888, pp. 8vo. 350.

The work is necessarily, from its brevity, synoptic in its treatment of the subject, and as such is especially adapted to instruct persons engaged in active life, whose time for reading is limited. To such persons we can recommend the work, as expressing in a brief space the results of recent research in a field of the greatest interest to all students of mankind or of our own part of it.—E. D. COPE.

Boulenger on Rhynchocephalia, Testudinata, and Crocodilia.[2]—In this book of 300 pages we have another valuable result of the labors of Dr. Boulenger, which will be of great utility to the students of the Reptilian orders named. The very full collections at the disposal of the author enable him to settle many questions of specific characters that have awaited elucidation, and to assign to their proper places in the system many forms which have been named. The system adopted is clear, and expresses the present state of our knowledge. For many of its good points the author is indebted to Dr. George Baur, whose recent researches in this field have been of great value. Dr. Boulenger has used Dr. Baur's observations with judgment, attaching values to them in accordance with their merits. We think he has undervalued the character of the mutual attachment of the plastron and carapace. On the other hand, the peculiarity of the nuchal bones has enabled Baur to distinguish the Dermatemydidæ from the Emydidæ. We do not now give as high a rank to the Athecæ as does Boulenger, nor would we abolish the suborders, as is proposed by Baur, but have adopted an intermediate course.

Dr. Boulenger reaches a remarkable conclusion as to the relations of the Trionychidæ with broad alveolar surfaces for crushing, to those with acute edges of the jaws. He finds that species of India, China, and Africa present individuals with both kinds of structure, which are otherwise undistinguishable, except by a few correlated characters. He thinks that the facts indicate a dimorphism in such species, one form being piscivorous and the other conchivorous. He has not found any young individuals with broad grinding alveolar surfaces, and suspects that that modification is acquired by the animal's entering on a diet of Mollusca, and that it is maintained by persistence in it throughout life. The genus Platypeltis thus becomes a modification of Trionyx. This is certainly a remarkable proposition, and it ought not to be difficult to prove or disprove it by observations on our *Trionyx ferox*, the only North American species supposed to present constantly broad and flat alveolar surfaces.

[2] Catalogue of the Chelonians, Rhynchocephalians, and Emydosaurians in the British Museum By G. A. Boulenger. Published by the trustees, 1889.

The whole number of Testudinata recorded by Dr. Boulenger is 195. Perhaps there are a dozen species omitted. Of the Crocodilia 23 species are recognized.—E. D. COPE.

Frazer's Mineralogical Tables.[3]—The object of Dr. Frazer's tables is to enable the students to determine the nature of the most important minerals without recourse to blowpipe analysis. As is well known, the first edition was a translation of Dr. Weisbach's "Tabellen." In the third edition but little of Weisbach's original descriptions remain. Except the plan, the entire contents of the little book are Dr. Frazer's, so that the volume as it now stands is essentially a new production, in which are incorporated many of the most important portions of Groth's chemical tables and Weisbach's latest determinative tables. The arrangement of the tables is briefly as follows: The minerals are divided in accordance with luster and streak. These groups are printed together, and in columns opposite the names of the various members of each group are given their hardness, tenacity, crystal system and habit, cleavage, chemical composition, density, paragenesis, and a few blowpipe reactions in rare cases. With its aid there should be almost no trouble in deciding the name of any common mineral. The tables are especially valuable for field determinations. They are bound flexible cloth, and are of a convenient size for carrying in the pocket.—W. L. B.

[3] Tables for the Determination of Minerals. By Persifor Frazer. Third edition. Philadelphia, J. B. Lippincott Company, 1891, pp. IX., 115.

General Notes.

GEOLOGY AND PALEONTOLOGY.

Geological Survey of New Jersey, 1890.[1]—This annual report covers the work done under the direction of Mr. I. S. Upson, assistant-in-charge under the late Dr. G. H. Cook, and the present state geologist, Mr. J. C. Smock. It includes the report of Frank L. Nason on the crystalline rocks of the Highlands, and of the magnetic ores of that district; Mr. Coman's notes on (1) the sediments overlying the upper marl beds in eastern Monmouth county, (2) evidences of former shore-lines above the present sea-level, (3) the gravel of the Trenton terrace; Mr. C. C. Vermeule's report on the observations of stream-flow and rainfall up to date; a paper from Mr. Lewis Woolman describing the artesian wells in the southeastern coast-belts of the state; and, finally, an account by G. W. Howell of the drainage work inaugurated by the survey and carried on with so much success and such beneficial results.

Mr. Nason's careful examination of the outcrops has resulted in the discovery of organic remains in the crystalline limestones, which have been referred by Prof. C. E. Beecher to the Cambrian, and below the Potsdam sandstone formation, as has been done by Mr. Lewis Woolman in Pennsylvania. His ideas concerning the limestones of Sussex county are summed up in the following conclusions:

"First, the white limestones of Sussex and Warren counties are of post-Archean age. Second, the white and blue limestones belong to a synchronous horizon. Third, this horizon is the horizon of the Olenellus fauna."

The extent of these conclusions is more far-reaching than is at first sight apparent. First, it demands that a careful search be made for fossils in the whole belt, or rather belts, of limestones, sandstones, slates and shales hitherto called and regarded as Potsdam, Trenton, and Hudson River. The result may prove the existence of a great horizon of rocks in New Jersey, New York, and Pennsylvania hitherto unsuspected, and may also throw much light on the question as to the position of the Green Pond Mountain rocks. Second, in this belt are rocks—limestones, sandstones, slates, iron and zinc ores—in every

[1] Geological Survey of New Jersey: Annual Report of the State Geologist for the year 1890.

degree of metamorphism. The belt is penetrated by various kinds of igneous rocks; and the petrography and chemistry of rocks and minerals in every stage of metamorphism, induced by pressure and heat, can be traced out and its history deciphered to its minutest details. It is rare to find rocks of known geological age in which such favorable conditions exist. The histories thus elaborated can be used most advantageously in deciphering other localities whose history is written in less legible characters.

Elevated Sea-Beach on Grand Cayman.—Southeast from the Isle of Pines, and distant about two and one-half degrees, lies the Grand Cayman Island. Politically it is a dependency of Jamaica, from which it is separated about as far as it is from the Isle of Pines. Geographically it seems to be more closely associated with Cuba than with Jamaica, in so far as any argument derived from the ocean depths around would indicate. The water gradually deepens from about three hundred and fifty fathoms at Cape Cruz (Cuba) to about eight hundred at Cayman Brac and Little Cayman; thence to over one thousand one hundred fathoms as one approaches the Grand Cayman. The course from Cape Cruz to the Grand Cayman is west by south. The distance is one hundred and forty nautical miles; and as the smaller islands of Cayman Brac and Little Cayman lie almost in the line indicated between Cape Cruz and Grand Cayman, and as the water deepens north and south of the line, one readily recognizes the presence of a submarine ridge stretching from Cuba to Grand Cayman, of which these Caymans are themselves merely coral-capped summits.

The main fact here indicated is the distance, and the depth of ocean which surrounds this island of the Grand Cayman; to the south of it two, and even three, thousand fathoms are found. During the past winter I made a brief visit to the southern shore of the Grand Cayman. My attention was called to a sea-beach or wall, fifteen feet above the present tide-level. The most casual observer could not fail to notice that it indicated the action of the water, and that the materials of the beach came from the water. The broken and worn masses of coral along this higher shore led to but one conclusion. I immediately began speculating on the time required to form that shore-line, and how long it took place, etc. The fact is, however, that the whole thing happened in a single day, during one of the fearful hurricanes which swept over the island in the earlier part of this century. The testimony on this point is quite too direct and positive to admit of any doubt.

816

PLATE XVIII.

STORM BEACH ON GRAND CAYMAN.

The vast depth of water to the south of the island, and the long unbroken sweep of the ocean in the same direction, prepare one to believe anything might be possible when that vast body of water was urged on by a tropical hurricane. The illustration shows the hurricane beach above and the present line of the water below. (Plate XVIII.)

The most obvious lesson of this special case is a due caution in reasoning on shore elevations, unless all the elements of the problem are absolutely known.—J. T. ROTHROCK.

New Acquisitions to the Eocene Fauna from Southern Patagonia.—In a letter dated May 5th, 1891, M. Florentino Ameghino informs us of the paleontological researches of his brother, Carlos Ameghino, on the borders of the river Gallegos, in Patagonia,—whose former researches we have already made known to our readers. The new results may be summed up as follows:

1. The lemurs (Prosimiæ) are for the first time found in the Eocene of Patagonia.

2. The discovery of new species of Microbiotheriidæ in good condition serves to confirm the opinion advanced by M. Ameghino in his last work on the Plagiaulacidæ. The Microbiotheriidæ have multiple incisors (four on each side), and so seem to represent the ancestral type of the Plagiaulacidæ and of the Diprotodont Marsupials.

3. The same formation furnished an entirely new group of Plagiaulacidæ with multitubercular molar teeth, but with the same dental formula as the species already known. The last molar is, however, entirely rudimentary. These new types connect the Plagiaulacidæ of Patagonia previously described with the Plagiaulacidæ of Europe and of North America. (*Revue Scientifique*, July 4th, 1891.)

The Progress in Geology for the Years 1887–1888.—In a résumé of the geological work done in the years 1887–1888, Mr. W. J. McGee cites the following as the most important:

1. The transition from an empiric classification in geology to a natural one by processes or by fundamental principles and laws.

2. The birth of the new geology, which interprets geologic history from the records of degradation, as the old geology did from the records of deposition.

3. The invention of a method of determining the depth of earthquake centers and of the velocity of earthquake transmission.

4. The recognition and definition of a great geologic group,—the Algonkian, and also of a subgroup of rocks,—the Lower Cretaceous, and the correct determination of the succession of the subordinate

divisions of the Silurian and Cambrian in the structurally complex field east of the Hudson River. (Smithsonian Report for 1888.)

We fail to see that Mr. McGee sustains the comprehensive claims made under the first and second heads. Both departments of geology referred to had been fully established prior to 1887!

Fossil Birds from the Equus Beds of Oregon.—In the AMERICAN NATURALIST of last April (1891) the present writer invited attention to the fact that he had in hand for description a large collection of fossil birds from the Upper Pliocene of the Silver Lake region, Oregon. A small part of these had been loaned by Professor Condon, of the University of Oregon, but the great bulk of the collection belonged to the cabinet of Professor Cope, who had with marked generosity placed them at my disposal for the aforesaid purpose.

My labors upon this fine collection have now been completed, and the work is ready to be passed into the hands of the printers and engravers. The memoir will make a quarto volume of upwards of a hundred pages, and is illustrated by four quarto plates, presenting over forty figures of the bones of the new species and genera.

The present notice pretends to be nothing more than a brief abstract, giving a list of the species described, with remarks thereon.

Those indicated by an * have already been either noticed or described elsewhere by Professor Cope.

PYGOPODES.

1. *Æchmophorus occidentalis.**
2. *Colymbus holbœllii.*
3. *Colymbus auritus?*
4. *Colymbus nigricollis californicus.**
5. *Podilymbus podiceps.**

LONGIPENNES.

6. *Larus argentatus smithsonianus.*
7. *Larus robustus.* Sp. nov.
8. *Larus californicus?*
9. *Larus oregonus.* Sp. nov.
10. *Larus philadelphia.*
11. *Xema sabinii.*
12. *Sterna elegans?*
13. *Sterna fosteri?*
14. *Hydrochelidon nigra surinamensis.*

STEGANOPODES.

15. *Phalacrocorax macropus.**
16. *Pelecanus erythrorhynchus?*

ANSERES.

17. *Lophodytes cucullatus.*
18. *Anas boschas.*
19. *Anas americana.*
20. *Anas carolinensis.*
21. *Anas discors.*
22. *Anas cyanoptera?*

23. *Spatula clypeata.*
24. *Dafila acuta.*
25. *Aix sponsa.*
26. *Aythya marila nearctica?*
27. *Glaucionetta islandica.*
28. *Clangula hyemalis.*
29. *Branta hypsibatus.**
30. *Branta propinqua.* Sp. nov.*
31. *Branta canadensis.**
32. *Anser condoni.* Sp. nov.
33. *Anser albifrons gambelii.**
34. *Chen hyperborea.*
35. *Olor paloregonus.**

ODONTOGLOSSÆ.

36. *Phœnicopterus copei.* Sp. nov.

HERODIONES.

38. *Ardea paloccidentalis.* Sp. nov.

PALUDICOLÆ.

39. *Fulica americana.**
40. *Fulica minor.* Sp. nov.

LIMICOLÆ.

41. *Phalaropus lobatus.*

GALLINÆ.

42. *Tympanuchus pallidicinctus.*
43. *Pediocœtes phasianellus columbianus.*
44. *Pediocœtes lucasii.* Sp. nov.
45. *Pediocœtes nanus.* Sp. nov.
46. *Palæotetrix gillii.* Gen. et Sp. nov.

ACCIPITRES.

47. *Aquila pliogryps.* Sp. nov.
48. *Aquila sodalis.* Sp. nov.

STRIGES.

49. *Bubo virginianus.*

PASSERES.

50. *Scolecophagus affinis.* Sp. nov.
51. *Corvus annectens.* Sp. nov.

Of the fifty-one species here enumerated ten have already been noticed by Cope, three of which were described as new, and one of which has since proved to be new,—*i. e.*, the brant I have described above as *Branta propinqua*.

Larus robustus is represented by a left coracoid, which is nearly perfect. This bone is shorter and stouter than the corresponding element in *Larus glaucus;* otherwise its characters are almost identically the same. *Larus robustus* was a gull rather smaller than *L. glaucus*, with which it may easily have been closely related.

Another gull was *Larus oregonus*, an extinct species new to science, which was about the size of *Larus delawarensis*, and its existence is based upon two humeri, both from the left side of two different individuals. The characters exhibited on the part of these bones are distinctly different from those of other gulls of this size. *Anser condonii* is a great extinct goose, vastly larger than our present existing Canada goose (*B. canadensis*). Its remains are represented in Professor Cope's collection by a fractured *os furcula* and the parts of two others. These were carefully compared with the furculæ of a number of our wild geese and swans of the genera Chen, Anser, Branta, Dendrocygna, and Olor, and it was found that, upon the whole, the majority of its characters agree best with *Anser albifrons*.

These characters and comparisons are dwelt upon in detail in my forthcoming memoir. I have named this goose after Professor Thomas Condon, of the University of Oregon, the first naturalist who discovered and collected any of the remains of fossil birds in the Silver Lake region of Oregon. A fine series of bones represents the new brant, *Branta propinqua*,—a small goose evidently referred to by Cope as a species that came "near *nigricans*." I propose the above name for this now extinct form.

One of the most interesting discoveries made in the Silver Lake region was the fossil remains of a new species of a now extinct flamingo, which I have called *Phœnicopterus copei*, in honor of Professor E. D. Cope, who discovered it. I found a number of bones belonging to this species in the collection, and a study of them reveals the fact that *P. copei* was a somewhat taller and longer-winged flamingo than *P. ruber*, though at the same time it was probably not quite so stout in the body.

A small heron, to which I have given the name of *Ardea palocci-dentalis*, and a new coot, *Fulica minor*, smaller than *Fulica americana*, were, as will be seen in the list given above, also among the novelties. The first-named was a species somewhat larger than *A. candidissima*, and smaller than *A. egretta*, with osteological characters identical with existing herons. Osteologically, too, the two coots agree, the species differing only in size.

It will be observed that the Gallinæ were very well represented in the avifauna of Western Oregon during the later Tertiary times, and among their fossil remains I found three species of them that proved to be new. Both of these were closely related to *Pediocætes phasianellus columbianus*. The larger form, *Pediocætes lucasii*, I have named in honor of my friend, Mr. F. A. Lucas, in recognition of his published labors in

avian osteology, and his past and present museum work, both in paleontology and bird-structure.

P. nanus was smaller than *columbianus*, and decidedly smaller than *P. lucasii*.

Another grouse was *Palæotetrix gillii*, a rare form apparently, and one smaller than a female Centrocercus, but considerably larger than the largest forms of Tympanuchus. It was probably related to several of the existing species, and may have been in the ancestral line of the sage cocks. Its remains exhibit osteological characters that differentiate it generically from our present tetraonine types. I have named it in honor of Dr. Theodore Gill, the eminent ichthyologist and biologist.

Two new eagles were also discovered, but their fossil remains were not abundant. *Aquila pliogryps* I believe to have been a large bird,—somewhat larger than *A. chrysaëtos*, to which it was apparently related. In its proportions it was apparently of a more slender build, however, and may have had habits akin to the falcons, and was at any rate a very formidable bird. *Aquila sodalis* was a considerably smaller eagle than *A. pliogryps*, though not much smaller than *A. chrysaëtos*. None of the lesser Accipitres were discovered.

Among the Passeres, I met in the collection with the remains of an extinct blackbird, which I have called *Scolecophagus affinis*, it being related to Brewer's blackbird, and probably in those Pliocene times resorted in numbers to the vegetal growth of the margins of the lakes. *Corvus annectens* was a raven, smaller in size than any of our present ravens, to which it was very closely related.

It is not my intention here to abstract any part of my "conclusions" in reference to the avifauna of the Equus beds of Oregon, as such remarks can well remain until the appearance of the general work upon the subject.

In closing, it gives me pleasure to tender my sincere thanks to Professor G. Brown Goode, of the National Museum, for the unlimited facilities extended to me in the matter of the loan of the skeletons of existing birds from the museum's collections; also to Mr. Lucas for his kindness in getting them to me after my request in that direction had been granted. That material, added to my own collection of bird skeletons, was ample for all purposes of comparison, for all the necessary existing species were at my command.—*R. W. Shufeldt, M.D., Smithsonian Institution, Washington, D. C.*

Geological News.—General.—Mr. L. J. Clark confirms Flemming's theory that the Island of Toronto is formed of material which came originally from the Scarboro' Heights, and that the mechanical

force which transported the material to its present resting-place was the storm action of waves. (Trans. Can. Inst., March, 1891.)——The most interesting fact developed in the recent surveys of the Pacific coast is that the coast-line of Southern California is more abrupt than that of any part of the Atlantic or other portion of the Pacific. (*Scien. Am.*, July 25th, 1891.)

Archean.—Professors Solas and Cole call attention to the streakiness which characterizes the interlamination of an olivine and coral sand-rock, and note its resemblance to eozonal and serpentinous limestone. (Proceed. Roy. Dublin Soc., 1891, p. 124.)

Paleozoic.—Sir William Dawson has described a new fossil plant, *Lepidodendron murrayanum*, from the Carboniferous rocks of Newfoundland. The specimen shows the character of the old stem, branches, and leaves. (Bull. Geol. Soc. Am., Vol. II., p. 532).——Rev. H. Herzer has found in the Upper Helderberg limestone, near Sandusky, Ohio, a fossil fragment of an Alga, which has been described by Prof. Lesquereux under the name, *Halymenites herzerii*. The specimen is remarkable, and of great value, from the fact that its internal structure is so well preserved that its characters are clearly discernible. One or two specimens only of that kind are recorded by paleobotanists (Proceeds Nat. Mus., Vol. XIII.)——Mr. H. M. Ami has contributed a paper to the *Canadian Record of Science*, April, 1891, in which he says that it is perhaps premature to state the precise geological horizon of the strata at Quebec city, but in his opinion they occupy a position in the Ordovician system higher than the Lewis formation, but lower than the Trenton, and are probably an upward extension of Sir William Logan's "Quebec Group." This would make them about equivalent to the Chazy formation of the New York and Ontario divisions.

Mesozoic.—M. Kilian and M. Leenhardt have decided that from a stratigraphical standpoint the sands of the valley of the Apt, in Southeastern France, are Cretacic, and not Tertiary, as has been thought. (Bull. No. 16, Tome II., de la Carte Geol. de la France.)——Mr. A. J. Jukes-Brown and Rev. W. R. Andrews have ascertained, by means of a well sunk at Dinton, Eng., that there is a well-developed Upper Purbeck series in the vale of Wardour, with a thickness of 70 or 80 feet, and this is succeeded by representatives of the Wealden and Vectian series, which, however, are poorly developed, and taken together are less than 100 feet. (*Geol. Mag.*, July, 1891.)——Mt. Diablo is an isolated peak of the Coast Ranges of California, lying about 27 miles

east by north of San Francisco. H. W. Turner reports that it consists of a central mass of metamorphic rocks. The strata immediately surrounding the metamorphic mass are, except for a space on the southwest, of Cretaceous age. Next to the Cretaceous, going away from the mountain in any direction, are Eocene (Tejon) strata, and these are followed successively by Miocene, Pliocene, and Plistocene deposits. (Bull. Geol. Soc. Am., Vol. II., pp. 385–414.)

Cenozoic.—M. Depèret has recently published a list of the fauna from the different Pliocene beds of Theziers. It contains 302 species, and many varieties, of invertebrates, comprising 112 Gastropods, 107 Lamellibranchs, 3 Pteropods, 5 Brachiopods, 3 Crustaceans, 4 Echinoderms, 7 Polyps, and 1 Bryozoan. This is much the most extensive that has been given for any single formation of the valley of the Rhone. (Bull. No. 6, Tome II., de la Carte Geol. de la France.)——M. Gaudry announces the discovery of mastodon remains near Chèrichira, in Tunis, probably the jaw of *M. angustidens*, similar to that of the Middle Miocene of Sansan. (*Rev. Sci.*, Ju 20th, 1891.)——In a recent paper (Bull. Wash. Philos. Soc., Vol. XI., pp. 385–410.) on the Mohawk beds in Eastern California, Mr. Henry Ward reaches the following conclusions: "The Mohawk valley is the bed of a Plistocene lake caused by the damming up of the cañon of the Feather River by a flow of andesitic lava. Glaciers existed contemporaneously with the lake."——According to R. Ellsworth Call, the Loess of Eastern Arkansas is Plistocene; the gravels and sands, Tertiary; and the lower clays, as indicated by the few fossils found, are Eocene Tertiary. The Loess about Helena is rich in fossil land-shells, but in general the fossiliferous exposures are few. Minerals of economic importance are not to be found, nor are the lignites of any importance from an economic standpoint. (Ann. Rept. Ark. Geol. Sur., 1889.) ——Recent researches by W. J. McGee have shown that the Appomattox formation consists of a series of obscurely stratified loams, clays, and orange sands, with local accumulations of gravel about waterways. It forms a widespread terrane, almost continuous with the Costal Plain between the Rappahannock and the Mississippi. No characteristic fossils have been found in it, but its stratigraphic position, unconformably below the Plistocene, and uncomformably above the Miocene, indicates an age corresponding roughly with the Pliocene. (Bull. Geol. Soc. Am., Vol. I.)

MINERALOGY AND PETROGRAPHY.[1]

Petrographical News.—In an argumentative article on the individuality of rocks Lang[2] proposes to regard as a rock individual the product of a continuous (uninterrupted) rock-forming process. Accordingly he would class interbedded clays, sandstones, and conglomerates deposited from the same body of water by a gradual lessening of its velocity, as a single rock, with clayey, sandy, and conglomeratic facies. In the eruptive group, that is a rock individual which has been forced from the depths of the earth by a single earth-throe. The beginning and end of a rock-forming process thus marks the limitations of a rock individual, even though a second period of similar processes should subsequently give rise to a rock of the same nature as that previously formed. The ideas discussed in the article are of great interest, but the practical advantages to be gained by regarding rocks from the point of view of its author are not immediately perceptible.—— Another article of some theoretical interest is Justus Roth's criticism of Rosenbusch's[3] recent paper on the chemical nature of eruptive rocks. After giving a short historical review of the attempt to explain the variation in eruptive rocks upon a chemical basis, the author[4] proceeds to examine critically the views set forth by Rosenbusch. In the first place, he states that ten of the sixty-three rocks whose analyses are quoted by this writer are much altered from their original condition; others are but local in distribution, while of others, again, the analyses are incomplete. He further continues by showing that the fundamental magmas by whose mixture the various types of rocks are regarded as made up, far from giving rise to definite varieties when mingled in definite proportions, may themselves consist of different combinations of different minerals. He then calls attention to what appear to be weak points in Rosenbusch's calculations, and concludes with the statement that there is not yet sufficient knowledge concerning the chemical character of eruptives to warrant the construction of a theory concerning their composition. ——Loewinson-Lessing[5] has attacked the chemistry of the eruptive rocks in a little different way from Rosenbusch. He compares the relations between the bases and the acid in a rock, and calls

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Maine.

[2] *Miner u. Petrog. Mitth.*, XI., p. 467.

[3] AMERICAN NATURALIST, 1890, p. 1071.

[4] *Zeits. d. deuts. geol. Ges.*, 1891, p. 1.

[5] Bull. Soc. Belge de Géol., etc., IV., 1890, p. 1.

those rocks acid that contain an excess of the latter,—*i. e.*, more than enough to saturate the bases present. This excess shows itself as free quartz. Thus it is not the percentage of silica in a rock-mass that carries it into the acid, neutral or basic class, but it is the lack or excess of silica as compared with the bases. Several empirical formulas represent the author's types. They are based on the determined ratio between bases and acid in neutral rocks, in which the amount of silica necessary to saturate the bases (in percentage) is represented by formula $SiO_2 = 2(R_2O + RO) + R_2O_3$. In neutral rocks this relation exists, consequently the formula represents the composition of such rocks. The amount of silica in basic rocks is represented by $SiO_2 = R_2O + RO + R_2O_3$, and in acid rocks $SiO_2 = 2(R_2O + RO) + R_2O_3 + Quartz$,—*i. e.*, the percentage of silica in these is greater than twice the sum of the percentages of R_2O and RO, plus R_2O_3, by as much as there is quartz in the rock. The author then uses these formulas, with others deduced from them, as a basis for the classication of eruptives. His paper is interesting reading, and if facts justify his formulas the conclusions reached by the author will prove of value to petrographers.——Some recent discussions on the pressure alteration of basic eruptives have added quite a little to our information regarding this phenomenon. Welch [6] has described the alteration of diabases into schists in the region of the Soonwalde, in the Taunus, on the left side of the Rhine. Well-developed diabases exist near Rauenthal. Under the influence of pressure they have given rise to schistose rocks containing actinolite, and others in which chlorite and epodite have replaced the original augite. Other schists, whose relationship to diabase can be determined only by a microscopical examination, have been called hornblende-sericite-schists, augite-schists, and sericite-calc-phyllites. The author has made a careful study of all these, which has resulted in their separation into schists composed of actinolite and epidote, those containing a blue amphibole, and finally those made up principally of chlorite. In some of these derived rocks augite may still be detected, in others the diabase structure is still visible, while in a third class no traces of the original constituents nor of their arrangement are recognizable. All show evidence of pressure in the shapes of their components and in their nature. In the first class the epidote and chlorite have been derived from augite. The second class contains, in addition to glaucophane, a bluish-green actinolite, sericite, and biotite. Epidote is entirely wanting. The rocks of the third group are combinations of chlorite, quartz, sericite, and generally calcite. In each the structure is schis-

[6] *Zeits. d. deuts. geol. Ges.*, XLI., 1890, p. 394.

tose, and the mother-rock was a diabase or a diabase-porphyrite. The paper concludes with a discussion of fifteen analyses of the rocks described.——Dynamically altered diabase and gabbros (sometimes schistose) occur also as sheets and dykes cutting altered sediments at St. Johnstown and Raphoe, in N. W. Ireland. Among them are epidiorites in which, according to Hyland,[7] the hornblende still preserves the ophitic structure of the original augite. The original feldspar was labradorite, but the changes effected in it have yielded a quartz-feldspar-epidote mosaic in which the secondary feldspar is oligoclase.——The schists, granulites, and even some of the massive rocks of the Lizard, England, are distinctly banded. To account for the phenomenon it has been suggested that it is due either to original sedimentation, or to the deformation of eruptive rock-masses, or to the injection of rock material along planes of weakness in preëxisting rocks. Since the schists are now known to be eruptive, the first explanation is not available. Against the second Somervail[8] brings the following objections, viz.: the symmetry of the banded structure, the frequent transitions between adjacent bands, and the uniform banding in large masses of the same composition. The injection theory is contradicted by the absence of irruptive contacts. He accounts for it on the supposition that segregations formed during the cooling of the magmas, yielding "schieren" that were afterwards squeezed.——A green schistose rock from near Zermatt, in the Pennine Alps, occurs so associated with other schists that Bonney[9] is compelled to regard it as a pressure schist derived from serpentine. The rock is so very fissile that it may be split into sheets one-eighth of an inch thick. Only two essential constituents are observed in it, one an olive-green mineral, occurring in small translucent flakes with a cleavage like mica and an extinction parallel to this, and the other a chromite or magnetite. The former mineral may possibly be antigonite. Associated with this rock are two other schists: one, a green schist, is a soft chloritic rock, composed of chlorite, magnetite, and zoisite (?). The composition of the chlorite is probably near that of chloritoid. The other schist is full of talc. The origin of neither of these could be determined.——A suggestion in explanation of the cause of the transitions sometimes seen between crystalline and clastic rocks has recently been offered by Prof. Pumpelly,[10] who believes that rock disintegration by weathering

[7] *Geol. Magazine*, 1890, p. 205.

[8] Ib., Nov., 1890, p. 509.

[9] Ib., Dec., 1890, p. 533.

[10] Bull. Geol. Soc. Amer., Vol. II., p. 209.

has, in many cases, given rise to thick accumulations of loose material, which has subsequently been used in the manufacture of the detrital beds. If the new beds were laid down under the influence of gentle currents their basal member would contain a large proportion of the material of the older rocks, and would thus simulate them to a greater or less extent. If this explanation is found to hold good, it will obviate the supposed necessity of regarding granite and gneiss as derived from fragmental rocks by some form of obscure metamorphism.——Rosenbusch [11] attacks the subject of the origin of the schists from a chemical standpoint. He shows by citing analyses that some foliated rocks have compositions corresponding to those of massive rocks. These he regards as squeezed eruptives. Others are different in composition from any known eruptive. These he regards as dynamically changed sedimentaries. The foundation principle underlying the argument is that dynamo-metamorphism affects but little chemical change in the material upon which it acts, except such as may be brought about by percolating water.

Mineralogical News.—The unique mineral *melanophlogite* has been made the subject of recent study by Friedel [12] and by Streng.[13] The former writer has examined the apparent cubes of the mineral in polarized light, and has found them to consist of six tetragonal pyramids, with their apices turned in and their bases forming the sides of the cubes. When heated the substance blackens and finally becomes opaque. Its density is 2.030–2.052, and the angle between contiguous cubic faces 91° 23′. The cubical cleavage reported by Lacroix does not exist. Some crystals appear to be simple cubes, while others are made up of many individuals, which in their section appear as fibers radiating from a center. They owe their form, consequently, not to pseudomorphism. They are pseudo-regular. A very careful analysis of these crystals yielded a result corresponding to $20SiO_2+SO_3$, with small quantities of $SrSO_4$ and carbon. The crystals are implanted in opal. Other crystals, very similar to those described above, are the pseudomorphs of quartz described by Mallard. They have not the same density as melanophlogite, nor the same optical properties. The formation of the latter mineral is ascribed to the action of SO_3 on quartz. The author also describes fibrous crystals formed by the grouping of hexagonal lamellæ with most of the properties of the

[11] *Miner. u. Petrog. Mitth.*, XII., p. 49.

[12] Bull. Soc. Franc. d. Min., XIII., 1890, p. 356.

[13] Ber. d. Oberhess. Ges. f. Naturw. und Heilk, 1890, p. 114. Ref. *Neues Jahrb. f. Min.*, etc., 1891, I., p. 18.

tetragonal melanophlogite. Analysis of these gave $SiO_2 = 93.2$ per cent., $SO_3 = 5.7$ per cent. This substance loses twelve per cent. of its weight on heating, while the tetragonal mineral loses six per cent. Streng declares that the mineral does not contain sulphuric acid, since it yields SO_3 only when treated with an oxidizing agent. Whether the sulphur is in the form of free sulphur or of an organic sulphide, could not be determined by him. The crystals, according to this author, are bounded by $\infty O\infty$ and $\infty O2$, and are probably pseudomorphs of opal and quartz after some unknown mineral.——A little more than a year ago the nodular rose-red *eudialite* of Magnet Cove, Arkansas, was described by Hidden and Mackintosh.[14] Recently crystals of the substance have been investigated by Dr. J. F. Williams.[15] These are tabular transparent or translucent rose-colored or crimson bodies traversed by cracks in all directions. They are hexagonal with $a : c = 1 : 2.1174$. OR, $\infty P2$, R, $-\frac{1}{2}R$, $-2R$ appear on one crystal, and on another occur in addition $-\frac{3}{11}R$, $\frac{1}{4}R$. $\frac{1}{8}R$ and $-\frac{3}{4}R^5$. The density varies between 2.804 and 2.833, and the double refraction is positive. It is one of the youngest of the constituents of the eleolite-syenite in which it occurs. The negatively refracting *eucolite*, of about the same composition as eudialite, appears in brownish-yellow crystals in the same rock. In it a cleavage parallel to OR is quite pronounced. OR, R, $-\frac{1}{2}R$, ∞R, and $\infty P2$ bound its crystals. Its specific gravity is 2.6244–2.6630, and its hardness 5–5.5. He thinks it may be an altered eudialite.—— The zinc-bearing *rhodonite* from Franklin and Sterling, N. J., has been reinvestigated by Pirsson.[16] The crystals are usually tabular and elongated, parallel to $\infty'P$. The new planes $2P_{\overline{\infty}}$, $4P_{\overline{\infty}}$ and $4P'$ were observed. The axial ratios, calculated from the best measurements obtained, are: $a : b : c = 1.078 : 1 : .6263$, $\alpha = 103° 39'$, $\beta = 108° 48' 30''$, $\gamma = 81° 55'$. The cleavage is prismatic, and the extinction on the basal plane is 54° from the edge $OP \wedge \acute{P}_{/}$. Since the crystallographic constants and the extinction of the mineral differ so slightly from those of rhodonite, it seems that the morphotropic action of the zinc is but slight. Composition:

SiO_2	FeO	ZnO	MnO	CaO	MgO
46.06	3.63	7.33	34.28	7.04	1.30

corresponding to $RSiO_3$.——Upon data collected during the course of many analyses of *uraninite* from American and foreign localities, Mr.

[14] *Amer. Jour. Sci.*, XXXVIII., 1889, p. 494.

[15] Ib., Dec., 1890, p. 457.

[16] Ib., XL., Dec., 1890, p. 484.

Hillebrand[17] concludes that nitrogen is a constant constituent of the mineral, and in a form different from any hitherto observed in the mineral kingdom; that its composition varies widely, and with it the physical characteristics; and, finally, that the composition does not correspond with any formula proposed to represent it. The author proposes to continue its study, and asks the aid of mineralogists in securing specimens.——Excellent transparent crystals of *anthophyllite* have been obtained by Mr. Penfield[18] from Franklin, Macon county, N. C. They are prismatic with ∞P and $\infty P\breve{\infty}$, but no terminations. The ratio $a : b$, based in approximate measurements, is .51375 : 1. The cleavage is perfect parallel to the prism and the brachypinacoid, and poor parallel to the macropinacoid, $2V_{na} = 88° 46'$, with the corresponding index of refraction for $\beta = 1.6353$; $a = A$, $b = B$, $c = C$. The double refraction is negative for green and yellow light, and positive for red light. Density = 3.093. The composition is:

SiO_2	FeO	MnO	MgO	CaO	H_2O	Al_2O_3	Loss at 100°
57.98	10.39	.31	28.69	.20	1.67	63	.12

——The so-called *perowskite*[19] from Magnet Cove, Ark., proves upon analysis to contain niobium and tantalum, and, therefore, to belong with dysanalyte, and not with perowskite. Its density is 4.18, and composition:

CaO	MgO	FeO	Fe_2O_3	$(Y.Er.Ta)_2O_3$	$(CeLaDi)_2O_3$	Nb_2O_5	Ta_2O_5	TiO_2	SiO_2
33.22	.74	.23	6.16	5.42	.10	4.38	5.08	44.12	.08

——Some new facts with reference to the *beryls*, *bertrandites*, and *phenacites* from Mt. Antero, Col., are communicated by Penfield.[20] The crystals occur implanted in granite or on the feldspar and quartz of this rock. The beryl probably afforded the beryllium for the other two minerals. It occurs in light-green or blue prismatic crystals, usually simple combinations of P and OP, but occasionally also with $2P2$ and $\infty P\frac{2}{3}$. The peculiarity of the crystals is the number of depressions observed on various planes. These are pits bounded by pyramidal faces, and into them long needles of beryl project. The forms on the needles are $12P\frac{6}{5}$, $12P2$ and $2P$. The axial ratio of the *bertrandite* is .5993, instead of .5953, as printed in the author's first paper[21] on the mineral. Some of the crystals of phenacite are inter-

[17] Ib., Nov., 1890, p. 384.

[18] Ib., Nov., 1890, p. 394.

[19] Ib., Nov., 1890, p. 403.

[20] Ib., 1890, p. 488.

[21] Ib., XXXVI., 1888, p. 52.

penetration twins, with the base the twinning plane.——*Harmotome* is announced by Ferrier[22] as occurring in calcite filling vuggs in veins cutting the Animikie slates in the vicinity of Rabbit Mountain, Ontario. Besides calcite, the minerals associated with the harmotome are amethyst, fluorite, and pyrite.——Black tin-bearing *rutile* crystals, supposed to be from the Harney Peak district, in the Black Hills, have a density of 5.294. Its composition, according to Headdn eand Pirsson,[23] is $TiO_2 = 90.79$; $FeO = 8.01$; $SnO_2 = 1.35$. The habit is orthorhombic, with $P\infty$, $\infty P\infty$ and P, twinned parallel to $11P\infty$.——Tin ore has not yet been found in workable deposits in Texas. Most of the material from that state reported as being cassiterite is keilhanite,[24] tourmaline, or black garnet.——Analyses of rhodochrosite from Franklin Furnace, N. J., yielded Browning,[25] after correcting impurities, the following figures:

MnO	CaO	ZnO	MgO	FeO	Fe_2O_3	CO_2	Sp. Gr.
45.55	11.41	1.48	1.78	.22	.16	39.40	3.47

Miscellaneous.—It is a pleasure to know that Messrs. Clarke and Schneider[26] are busy with experiments looking toward the settlement of problems relating to the constitution of natural silicates. The methods employed by them resemble somewhat those that have afforded such excellent results in organic chemistry. Silicates are exposed to the action of hydrochloric acid gas at high temperatures, and to its solution at ordinary temperatures, and the effects in each case are noted. They are then ignited and subjected to other reagents, and the products here obtained are compared with the results of the former experiments. In this way the presence of groups of elements is recognized that are analogous to the residues of carbon chemistry. The conclusion already arrived at by the authors are to the effect of *talc* is an acid metasilicate ($Mg_3H_2(SiO_3)_4$), and not a basic salt of pyrosilicic acid ($Mg(Si_2O_5)_2(MgOH)_2$) as Groth suggests. *Serpentine* is thought be a substituted orthosilicate corresponding to $Mg_2(SiO_4)_2H_3(MgOH)$, and its relation to olivine and chondrodite are represented as follows: Olivine $= Mg_4(SiO_4)_2$; chondrodite $= Mg_3(MgF)(SiO_4)_2$; serpentine $= Mg_2(MgOH)H_3(SiO_4)_2$. *Chlorite* is regarded as $R''_2(SiO_4)_2R'_4$, or olivine with half the magnesiem replaced by R′, and the fluoriferous

[22] Ib., Feb., 1891, p. 161.

[23] Ib., March, 1891, p. 249.

[24] Ib., March, 1891, p. 251.

[25] Ib., Nov., 1890, p. 375.

[26] Ib., 1890, XL., p. 312, 405.

phlogopite from Burgess, Ont., as $Al(SiO_4)_3Mg_3KH_2+Al_2(SiO_4)_3Mg_3K(MgF)$.——Belowsky[27] finds that green hornblende, when subjected for some time to the heat of a bunsen burner, changes its color, the character of its pleochroism, the strength of its double refraction, and the position of its planes of extinction, and becomes in all respects similar to the basaltic variety. Upon the addition of a little sodium chloride[28] to a solution of sodium sulphate, the latter salt crystallizes upon evaporation as *thenardite*, and not as mirabilite. A mixture of potassium and sodium sulphate under the same conditions yield *glaserite*.——A few illustrations of regular growth of pyrite crystals are given by Smolar.[29] Some of the crystals (all of which are from Piedmont and Elba) are probably interpenetration twins according to some new law, while others are regular growth only. The author believes that our knowledge of twinned crystals is still in its infancy, and advises many more observations on regular intergrowths before we conclude as to the conditions of twinning. Rhombic pyramids of sulphur[30] crystallize from picoline or pyridine saturated with sulphuretted hydrogen.——The *oölitic iron ores* of the Cliniton group in the Alleghenies are not cemented concretions, as was supposed, but they are pseudomorphs of bryozoons, or of fragments of these.[31]

ZOOLOGY.

Sipunculus gouldii.—Dr. E. A. Andrews, in a paper[1] that should have received earlier notice, describes the structure of this common east coast Sipunculid. The principal points brought out are the rich peripheral nervous system; the presence of "pseudostomata" in the splanchnic epithelium, the function of which remains unsettled; the division of the alimentary tract into several regions, differentiated by anatomical and histological peculiarities; the probability that the stomach is a sucking organ; the confirmation of the discovery that the reproductive organs were on the posterior surface of the origin of the posterior retractor muscles. Andrews thinks that the points which he has

[27] *Neues Jahrb. f. Min.*, etc., 1891, I., p. 291.
[28] Retgers. Ib., 1891, I., p. 276.
[29] *Zeits. f. Kryst.*, XVIII., 1890, p. 475.
[30] Ahrens. *Ber. d. deutsch. chem. Ges.* XXIII., 1890, p. 2708.
[31] *Amer. Journ. Sci.*, Jan., 1891, p. 28.
[1] Studies Biol. Lab. Johns Hopkins Univ., IV., p. 389, 1890.

brought out emphasize the similarities which exist between the Sipunculids and Phoronis. He also thinks that in spite of the absence of metamerism there is nothing in the anatomy of the adult which could not be explained on the theory of lost segmentation in these worms, while he institutes a striking comparison between the gonads of the Polychæte placed on the posterior side of a dissepiment and those of Sipunculus attached to the posterior surface of the pharyngeal retractors.

Sipunculus nudus.—H. B. Ward describes[2] the anatomy and histology of this Mediterranean form. The material at command embraced only the anterior portions of the body, so no study was made of many features worked out by Andrews. The histology of the body-wall and of the nervous system receive most attention. Among the many points brought out, the most interesting is the existence of a "cerebral organ," consisting of a canal opening to the exterior in the dorsal median line, and connecting posteriorly with an area in close connection with the brain, which the author is inclined to regard as sensory, and to homologize with similarly situated organs in Phymosoma and *Sipunculus gouldii*. The general accounts of structure given by Andrews and Ward agree well in broader features, but differ considerably in detail. From the evidence afforded by the nervous system Ward is inclined to the view that there is only a remote relationship between Sipunculus and the Annelids.

The Eye in Blind Crayfishes.—G. H. Parker has studied[3] the eyes in *Cambarus setosus* and *C. pellucidus*. In both species the optic ganglion and optic nerve are present, the latter terminating in the hypodermis. In *C. setosus* the retina has almost completely degenerated, while in *C. pellucidus* it is represented by a thicker portion of the hypodermis with scattered multinuclear granulated bodies. These Parker regards as degenerated clusters of cone cells. Thus *Cambarus setosus* from the caves of Missouri has gone farther in the line of ophthalmic degeneration than its ally from the caves of Kentucky and Indiana.

"**The Compound Eye of Crustaceans**" is the title of a larger paper[4] by the same author. He gathers together all that is known, adding much himself in the endeavor to solve certain problems surrounding these organs. The retina of the compound eye is composed of a number of similar units or ommatidia, but the structure of the ommatidium varies considerably in different groups. One question

[2] Bull. Mus. Comp. Zool., XXI., No. 3, 1891.
[3] Ib., XX., No. 5, 1890.
[4] Ib., XXI., No. 2, 1891.

which Parker asks is: "What are the means by which ommatidial types are modified, and what is the significance of the changes through which these types pass?" The argument of the present paper is that the ommatidia composed of four cells are more primitive than those containing many cells, and to show how cell-division can have caused the modifications and what other factors may be concerned.

Parker recognizes three types of compound eye so far as retinal structure is concerned: I., represented by Decapods, Schizopods, Stomatopods, Isopods, Leptostra, and Branchiopodidæ of the Phyllopods, has the retina a simple thickening in the superficial ectoderm; II., in Apodidæ, Estheridæ, and Cladocera, the retina does not retain a superficial position, but becomes covered by a fold of the integrement, the pocket remaining open in some and closed in others; III., in Amphipods, and possibly Copepods, the retina is completely separated from the hypodermis, not by an infolding, as in the second type, but by delamination. Of this the author is not certain from actual observation, but he gives a number of facts which warrant his conclusion. Types II. and III. pass through a stage comparable to type I., and hence may be regarded as derivatives from it.

Detailed descriptions of the structure of the ommatidia in various forms are given and illustrated by numerous figures. Continuing his work with that of his predecessors Parker tabulates the ommatidia of the different Curstacea in a manner which conveys at a glance the difference between them so far as numbers of cells are concerned. In an ommatidium going from the surface to the optic nerve there are present (1) the cells of the corneal hypodermis which secrete the cornea; (2) the cone cells, varying in number from two to five, one function of which is to form the crystalline cone; (3) the retinular cells, which may be either undifferentiated or divided into two groups, proximal and distal, the total number varying between three and nine; and (4) accessory pigment, cells which are either of ectodermal or mesodermal origin, all of the rest of the eye being clearly ectodermal.

All compound eyes of the Crustacea are thus clearly built upon a common plan, and the variations between them are traced to (1) the differentiation of cells, (2) the suppression of cells, and (3) probably to multiplication of cells by division, although this last has not been demonstrated by observation.

Parker traces the origin of the ommatidium to such structures as are seen in the Chætopod Nais, where there is an eye composed of a few large transparent cells, the distal portions of which are in part covered

by pigment cells. The transparent cells are the forerunners of the cone cells, while the pigment cells represent an as yet undifferentiated retinular region. This view is clearly opposed to that of Watasi, already noticed in our pages, by which each ommatidium is regarded as an involution of ectodermal cells, the members of which may still retain their power of secreting cuticular structures. The ommatidium, on the contrary, is a differentiated cluster of cells in a continuous unfolded epithelium. Patten's view that the ommatidium is a hair-bearing sense-bud receives no confirmation in Parker's studies.

Eyes in Arcturus.—Beddard describes[5] the eyes in deep and shallow-water species of Arcturus. He finds that some at least of the deep-sea forms show evidences of degeneration in the visual apparatus. Thus in three species he finds the conical lens showing signs of becoming opaque, while in three others there is alteration of size and curvature. In several others the rhabdom affords similar evidence, while in many deep-sea forms the decrease in amount of pigment is also regarded as an indication of degeneracy.

The Northern Limits of the Scorpions.—During a recent collecting trip to the Bad Lands of the Hot Creek Valley, in the extreme northwestern corner of Nebraska, Mr. F. C. Kenyon, a member of the party, found a living scorpion (Buthus sp.). This is, so far as I am aware, some three hundred miles further north than any member of the group has ever been found east of the Rocky Mountains. West of the mountains, I am informed by Mr. Henshaw, they extend north to Oregon and Washington.—J. S. KINGSLEY.

Glands in Orthoptera.—H. Garman describes, in the current volume of *Psyche*, peculiar glandular structures borne between the ninth and tenth terga of the abdomen of the cave cricket. Their function is unknown; but the author points out the fact that scent glands occur in the females of certain Bombycids. Apparently he is not familiar with the recent literature of abdominal glands in the Hexapoda.

Notes on Tunicates.—Herdman thinks[6] that the usual divisions of the Tunicates, while convenient, do not express the true relationships of these forms. According to him, the Synascidiæ are in reality a polyphyletic group, the members of which touch the Ascidiæ simplices at various points. Hence a strictly phylogenetic classification will

[5] Proc. Zool. Socy. London, 1890, p. 365, pl. XXXI.

[6] *Nature*, June 11th, 1891.

ignore the Ascidiæ Compositæ of our text-books, although the division may be retained for convenience.

Walter Garstang[7] gives the first part of a review of the Tunicata of Plymouth, England. Species of the families Clavellinidæ, Perophoridæ, and Diazonidæ are enumerated and described at length. Pycnoclavella is a new genus.

W. A. Herdman promises a monograph of the British Tunicates.

The Affinities of Polypterus.—H. B. Pollard has studied some points in the structure of Polypterus which, he thinks,[8] throw light upon the relationships of this form. The ear, except in lacking the ductus endolymphaticus, is distinctly Urodele in its character, as is shown by the immense sacculus, which reaches the level with the semicircular canals, and by proximal division of the auditory nerve. The so-called opisthotic corresponds to the Urodele petrosum; the opthalmicus profundus and superficialis pass through a foramen into the nasal capsule; the supracranial fontanelle is partially roofed over by cartilage; the "paired vomer" is really dermopalatine, and corresponds to the so-called vomer of the Batrachia. The jaw muscles correspond to the Batrachian rather than the Teleostome type, while the nervous system corresponds almost to detail with that of larval Salamandra. The conclusion to be drawn from these facts is that the ancestry of the Urodeles must be sought in Crossopterpgians. Turning now to the affinities of the other piscine forms, Pollard promises to show homologies of Polypterus with the Stegocephali, while the position of the Dipnoi and Holocephali must be different from that usually accorded them. Chimæra, etc., are Dipnoan forms which have lost their dermal bones, and are retrograded in some respects toward the Selachian type. If the Dipnoan forms are to be regarded as ancestors of the Batrachia, we should expect to find in the larval Urodeles the same relations of quadrate and palatines to the cranium, but such is not the case. In the young Cryptobranchus the quadrate has nearly the same relationships as in Polypterus. The suprapharyngobranchial I. articulates with the auditory capsule, and from it the author traces the origin of the Urodele stapes. This paper is a sample of the errors into which it is easy to fall in ignorance of the facts of paleontology.

The Pineal Organ of Ichthyophis and Protopterus.—Burckhardt thinks[9] that all previous authors have mistaken an arterial

[7] Jour. Marine Biol. Assn. United Kingdom, 1891, Vol. II., p. 47.

[8] *Anat. Anzeiser*, VI., 338, 1891.

[9] *Anat. Anzeiger*, VI., 348, 1891.

plexus for the pineal organ in these forms. In Ichthyophis, on account of the enormous folding of the arterial plexus the roof of the thalamencephalon is completely covered. It falls into an anterior wall, embraces the ganglia habenulæ on either side, and a posterior wall between the posterior and superior commissures. At the spot where these swellings meet is a small pear-shaped vesicle, which extends into the space between the hinder part of the arterial plexus and the roof of the thalamus. Development shows that this is the pineal organ. In Protopterus the pineal organ is a small, irregular sac, which extends directly upwards from the boundary between the mid- and twixt-brains into the abundant connective tissue of that organ. Burckhardt has not yet traced its lumen into connection with the third ventricle.

Fœtal Period of the Seal.—Grieg has studied the period of pregnancy of *Phocæna communis*.[10] He analyses the known literature, and finds great diversity of opinion. He then studies thirty-five embryos, with dates of capture of the mother, and comes to the conclusion that the period is about nine or ten months. According to the Norwegian fishermen, copulation takes place usually in July and August; but he finds that the breeding season may extend, according to locality, from July to October, and the young are born from March to June. At the time of birth the young measure from 700 to 860 millimeters, and may reach a length of 880 millimeters.

Extent of the South American Fresh-Water Fish Fauna.—There are more fresh-water fishes in the neotropical than in any other region. A comparison made by Carl and Rosa Eigenmann of the latest lists of European and North American fresh-water fishes, with a list of the South American species, shows the extent of the South American fauna. In summing up the species they find 126 European, 587 North American, and 1,147 South American species. (Proceeds. U. S. Nat. Mus., Vol. XIV., pp. 1–81.)

The Chromididæ of the Fresh Waters of Madagascar.—One of the most interesting peculiarities of the fresh-water fish fauna of Madagascar is the presence of the Chromididæ, coincident with the absence of the Cyprinidæ and the Characinidæ, although these two groups are found in Africa, and the great paucity of Siluridæ, so that the Chromididæ are, so to speak, the only fresh-water fishes of this large island.

At the present time the Chromididæ are a family of South America and of Mexico, 230 species out of 260 having been described from that region.

[10] *Jena Zeitschr.*, XXV., 544, 1891.

Known for some years past by a single species referred to one of the most widely distributed forms in Africa, *Chromis niloticus*, the Chromididæ are to-day represented in Madagascar by nine series belonging to four genera, the affinities of which are as follows:

The Paretroplus are distinguished from the Hemichromis, which belong to Africa and to Asia Minor, by a greater number of anal spines. The Paretroplus belong to an African type.

If the Paratilapia have certain affinities with the Hemichromis, they are still more closely allied to the Acara, from which they are distinguished only by the indentations of the bony projections of the external branchial arch; but the Acara belong to tropical South America.

The Paracara also have close affinities with the Acara.

As to the Ptychochromis, Steindacher has shown that they are separated from the Chromis by the presence of a lemelliform, compressed projection from the upper part of the first branchial arch. This character is found in the genus Geophagus, from South America.

The genera of the Chromididæ peculiar to South America have ctenoid scales, while those of Africa and Western Asia have cycloid. Among the Chromididæ of Madagascar, the Paretroplus, we may say, belong to the African type, as they have cycloid scales. All the other Chromididæ of Madagascar have ctenoid scales. Thus the Chromididæ of Madagascar are more closely related to the fresh-water species of tropical South America than to the African species.

From a study of the herpetological and ichthyological fauna of the fresh waters of Madagascar, it appears that this island, Southern Africa, and South America formed in a pre-Tertiary epoch, parts of the same continent, which had a fauna of the same origin and character. While South America and Africa have received since the Pliocene epoch invasions of animals of another creation, Madagascar has had, on the contrary, no connection since that epoch with any other land, and presents to-day the same fresh-water fish fauna as it did in the Middle Tertiary, without any addition save that of the Carassius recently introduced. The division between Madagascar and Southern Africa must have happened before the commencement of the Miocene, for neither Cyprinidæ or Characinidæ are found in Madagascar. Communication must have lasted for a much longer time between South America and the South of Africa, which, toward the Pliocene epoch, had received an influx of Characins; whilst the Cyprins, introduced into Africa from the European continent, or rather from the South Asiatic, had hardly penetrated South America. (Dr. H. E. Sauvage, Bull. Soc Zool. de France, 1891, p. 190.)

EMBRYOLOGY.

Notes on the Development of Engystoma.—This interesting Batrachian occurs in the Piedmont section of North Carolina, near Littleton, and within twelve miles of the southern boundary of Virginia. Its presence after a rain may be discovered by its peculiarly plaintive note. Oviposition seems to occur in the evening and during cloudy afternoons. It is now late in July, yet two lots of ova have been found by me which had been very recently deposited by the parent female "frog-toad," as it is known here amongst the natives. The eggs are heavily pigmented at the upper or animal pole, being darker than the eggs of Rana, and also considerably smaller. They are laid in strings, but so coiled as to form a nearly complete single layer over a considerable surface of water. The gelatinous coating, as in Rana, spreads out under the surface of the water, where by its adhesion to the layer of molecules at the surface a certain amount of support is thus gained for the eggs. It is therefore evident that surface tension is an important agent in keeping the eggs of this genus, and those of Rana, at the surface of the water. Other genera, such as Bufo, do not have the eggs supported on the surface, but are laid in strings formed of one row of eggs, wrapped in a gelatinous cord which lies on the bottom of the pond in which oviposition occurs. Still other forms have the eggs glued together in large masses and supported upon water weeds; this is notably the case with some Urodeles, such as Amblystoma.

The development of the eggs of Engystoma is rapid; three days after deposit the larvæ escape from the egg-envelopes. Throughout the course of development there is well-marked evidence of geotropism, or of the action of gravity in maintaining the equilibrium of the egg. The animal or black pole remains uppermost, the heavier or light-colored vegetative pole remains lowermost; the whole egg is thus maintained in a position of static equilibrium with the earth's center. There seems to be no tendency to rotate the egg through ciliary action, previous to the closure of the medullary folds. That cilia are entirely absent on the eggs of Engystoma is proved by the fact that at the time the medullary groove is still open every egg of the same age is in exactly the same position in respect to the center of the earth, and remains so for a long time, or until the tail fold is well developed and the medullary groove has been closed. Before the closure of the

medullary groove, but after the egg has begun to elongate and the paired, secretory, adhesive surfaces of the under side of the head have appeared as rudiments, the position of the animal and vegetative poles is still the same as in the undeveloped ovum. The head end of the egg is slightly elevated above the caudal end. This is due to the forward growth of the head, and the retention of the heavier yolk, farther backward under the posterior half of the medullary groove. The medullary groove thus comes to be inclined downward a few degrees from the head toward the tail, but the groove looks exactly toward the zenith, while the yolk looks downward in every egg, even the inclination of the medullary groove with respect to the horizon being the same for every egg of the same age.

At this stage I neglected to note an extremely important fact,—viz., whether the cephalic and caudal poles of the same row of eggs were all of them lying in the same direction. The fact that no change of position occurs for a long time in the eggs of Engystoma would indicate that possibly we might find that the future cephalic pole of the egg bore a constant relation to the cephalic pole of the parent Engystoma, such as is known to be the case in *Batrachus tau*. Such relations between parent and offspring exist to a marked degree, if they are not universal, in plants, and it is desirable to know to what extent the same rule holds with respect to animals. According to what has preceded, the early development of Engystoma is peculiarly favorable for the purpose of testing the theory that the cephalic and caudal polarities of the parent are transmitted directly to the offspring, or that the future long axis of the embryo already conforms, even in the egg, to that of the parent.

The next step in the development of Engystoma is somewhat similar to that of Rana. As soon as the larvæ have the tail-fold well developed they turn over and lie on the side, curved upon themselves, within the egg-envelope. This is the condition of the eggs on the second and early part of the third day. On the third day the larvæ leave the egg, and then tend to fall upon the bottom of the pool or receptacle in which hatching occurs. Soon after this they begin to swim about actively, and, singularly enough, instead of swimming like a fish, for some reason, which it is difficult to make out, the larvæ revolve on their own long axes. This singular mode of locomotion is probably due to the peculiar manner in which the tail is vibrated. This mode of swimming lasts about a day, after which the larvæ begin to swim in the usual fish-like way. At this stage, when the larvæ come to the surface, the head is in contact with the surface of the water, and

when quiescent the axis of the body, when in a condition of equilibrium, assumes an angle of about eighty degrees with the surface. The adhesive organs near the mouth now become functional. Up to this time the light area on the yolk is prominent, and enables one to watch the singular rotation of the larvæ. The head now begins to widen rapidly, and the light area on the belly becomes darker. The tail-fold soon becomes very thin, and bordered all round by a delicate edging of black pigment. The larvæ cease to rotate on the fourth day, and no longer take up their angular position at the surface of the water, and now behave very much like the larvæ of Rana. They are now very heavily pigmented over the whole of the body; the light area over the space where the yolk was formerly placed has disappeared, and the larvæ are now black as seen from above.—J. A. RYDER.

ARCHEOLOGY AND ETHNOLOGY.[1]

The International Congress of Anthropology and Prehistoric Archeology of Paris. (*Continued from page 768.*)

Seventh Question: "Ethnographic Survivals Which Can Throw Light Upon the Social State of the Primitive Population of Central or Western Europe."

M. Hassler, of l'Assomption, opened the discussion of this question with a general presentation of the subject.

M. Lumholtz, of Christiania, Norway, gave a long and interesting description of his four years' residence in Australia.

M. Glaumont, of Bourail, presented the result of his studies of the usages, customs, and manners of the neo-Caledonians. M. Glaumont presented a series of fetiches which were used by the people.

Signor Belucci, of Perugia, presented a catalogue of his grand collection of amulets displayed at the exposition.

Dr. Hamy recalled a discussion which had taken place at the Society of Anthropology upon the subject of the savages of Mariannais, whom it was sought to prove were men living in ignorance of the use of fire; but Dr. Hamy presented a large fragment of pottery which had very evidently been made by fire and subjected to its use in cooking or otherwise.

M. Chily Naranjo, from the Canary Islands, recalled the conclusions which had been presented to the congress of 1878 by M. Verneau

[1] Edited by Dr. Thomas Wilson, Smithsonian Institution.

upon the subject of anthropology of the Canary Islands. He ranged himself on the side of M. Verneau in the belief that the Guanche population of these islands at the moment of their discovery were of the race of Cro-Magnon, and had come to a slight degree of higher civilization.

M. de Zmigrodski, of Cracow, presented a large chart of the objects which had been exposed at the exposition, containing the designs of more than 300 objects on which were the sign of the *svastika*, or ancient cross. He divided these into five parts: Asia Minor and its influences, the Greco-Roman epoch, Christian epoch, the prehistoric in Europe, and the contemporaneous of the nineteenth century. Several members expressed doubt as to the conclusions of this gentleman.

M. Dumoutier presented to the congress the costume of a woman of Muong, of Western Tonkin, which was ornamented with *svastikas*.

Mr. Jammes, of Realmont (Tarn), had spent many years in Cambodia, and had made many and extensive excavations in his search for evidences of prehistoric man in that country. He presented many of the objects which he had found and brought back. The collection was purchased by me for the United States National Museum; and these objects, with the locality whence they came, were described by me in the NATURALIST for March, 1890, p. 286.

M. Belucci announced the discovery of a number of flints chipped by intention. The discovery was made by MM. Cuchi and Biauchi, Italian travelers, in a locality named Denghis, in a high valley of the Abai Abyssinia. These chipped flints were in every way comparable to those of the prehistoric ages of Europe.

Eighth Question: "To What Extent do Archeologic or Ethnographic Analogies Sustain the Hypothesis of Relations or Migrations Among Prehistoric Peoples?"

M. Ernst, of Caracas, opened the discussion by a memoir on the ancient inhabitants of Merida, in Venezuela. The author occupied himself especially with small vases in the form of cuvettes, with legs, the extremities of which were joined between themselves by transverse pieces which formed a square, with angles more or less round or occasionally a circle. M. Ernst described three known types of these vases, their ornaments in relief, their colored designs, and gave reasons for the belief that these were objects of luxury, possibly of religion or cult. The distribution of the original type in Venezuela, in Costa Rica, and possibly in the countries of New Grenada

and Chiriqui, appears to assign to these ancient populations a community of origin.

Their language seems to confirm these ideas. The currents of the dispersion of this human family should have taken rise in the center of South America, from which they were directed from the north and the northwest.

M. Verissimo, of Para, said: "There are two Indian families in juxtaposition in Brazil. One of these is the Tupi Guaranis, who speak the *lingoa geral;* the other is that of the Tapouis,—that is to say, the barbarians, to which belonged, possibly, the men of Sambaquis."

The pottery discovered in the Isle of Marajo, at the mouth of the Amazon, and of which M. Verissimo presented a remarkable specimen, appeared to indicate other affinities than those from the north. The prehistoric men of Marajo had probably come from Central America, and followed the coast of the Atlantic. Among the discoveries which tend to confirm this hypothesis, M. Verissimo cites the implements of wrought jade which were met with in Brazil, and of which he presented a specimen sculptured in the form of a batrachian or frog.

M. Hamy observed that the figure of the object presented by M. Verissimo was sufficient to justify the supposition of a northern or western origin, as had been attributed to it by Verissimo,—that is, in the Antilles on the one part, in the Cundinamarca and Central America on the other, where abound those representations of the frog which have a role so important in the mythologic iconography of Central America. The representations of the frog in the New World authorizes the formula adopted by this question, "The Hypothesis of Prehistoric Migrations."

M. L. Netto, of Rio Janeiro, had some words to say upon the communication of M. Verissimo, upon the Sambaquis or shell-heaps of Brazil, and presented a number of objects found during their excavation, and particularly a grand fetich of the form of a fish, others of mortars in the form of fishes and birds, which could be nothing else than the work of actual savages. As to the question of the expansion of jade or jadeite in South America, M. Netto was of the opinion that it is a phenomenon that has not yet been satisfactorily explained.

Baron de Baye recalled and recited the theory adopted by Professor Putnam as to the Asiatic origin of the mineral of which these objects were made in Central America.

Dr. Gosse, of Geneva, was of the opinion that the question now under discussion was not more advanced in America than in Europe,

and that the origin of these hard stones employed in Europe during the age of polished stone is as yet undecided and much to be discussed.

Mr. Thomas Wilson described how the nephrite implements were used in Alaska, and how the mineral of which they had been made had been found by Sir George Dawson, in the form of smooth and worn pebbles, in the valleys of the Lewis, the Kowak, and the Yukon rivers. He said that Dr. Gosse was right to advise us to wait for further information. The elements of jadeite and nephrite were quite common. Why was it that these rocks should not be found in America as well as in Europe?

Mr. George F. Kunz, of New York, presented to the congress a votive hatchet in jadeite, beautifully sculptured, of extremely large dimensions, from Canada. It was his opinion that this mineral came from the southern part of Mexico; but the Mexicans of modern times had never yet discovered its origin. It might be perhaps upon the summit of some mountain. He continued by giving a description of several new minerals, similar to jadeite, which had been used in America for such implements, and have been identified by Professor F. W. Clarke as pectolite, wollastonite, pagolite, and agalmatolite. Implements made of these minerals have been found in divers portions of the United States of America, and therefore there was nothing curious or wonderful in finding the origin of the jadeite and nephrite in that country.

M. Netto presented a portrait of a woman of the tribe of Indians of Boticude, and with it gave a description of what was called the *botoque*, which much resembles, if it was not actually, a labret; and he said that many American people used it in distant portions of the country. In this he was supported by Dr. Hamy, who presented several curious specimens from his museum of ethnography, and especially those from the northwest coast of America. Both Dr. Hamy and M. Netto were, however, in doubt whether these objects would show a relationship between two peoples so widely separated.

Mr. Thomas Wilson instituted a parallel between the paleolithic period in the United States and that in Europe. This period has been manifested in the United States of America by implements found deep in the river gravels at three places or more at great distances from each other,—the Delaware, the Miami, and the headwaters of the Mississippi Rivers. He described these localities and the conditions of and associations in which the paleolithic implements were found,—likeness between them and paleolithic implements in the river gravels in Europe. Their appearance, mode of manufacture, were much the same. The difference

between them was principally that of material, and this was in some cases much the same; for argillite implements were found at Trenton, while the quartzite implements of Piney Branch would compare in all respects with those found by MM. d'Ahemar, Noulet, and Cartailhac in the valley of the Garonne. Mr. Wilson continued his discourse at some length by the comparison of the neolithic types from America with those of Europe. Many of both kinds were shown, some by the original and others by photographs and drawings.

M. Verneau discovered in the islands of the Canaries the polished stone hatchets of which the material, form, and work recalled those which were found in the Antilles, and, above all, at Porto Rico. There were other things of the same order which showed analogies, particularly the *pintaderas* of the Canaries, with the imprints of the ancient Mexicans.

M. Cartailhac gave a resume of the researches which he had just terminated upon the prehistoric archeology of the Balearic Isles, and proved that all the affinities of the archipelago were with the south. He had not found a trace of the age of stone, and the grand monuments had an African aspect or appearance.

M. Tardy dilated upon the beginnings of civilization in Algeria, and the synchronisms of the first stages of the earlier ages of humanity in Africa and in Europe.

M. Belucci presented a note upon the relations between the central and southern portions of Italy at the epoch of the polished stone. These relations are established not only by the obsidian of Lapiri, which came from Central Italy, but also by the particular forms of arrow-heads, and by the scrapers and knives of a variety of flint which had its origin in the southern provinces; finally by the polished stone hatchets of the form *bombé*, which were common in Southern Italy, and which were met, though rarely, in Central Italy.

M. Adrien de Mortillet communicated a summary of the results of a mission which had been confided to him by the Commission of Megalithic Monuments: to study the monuments of the same nature in Algeria, and to compare them with those of France.

M. Hamy did not agree with these conclusions, and he commended conservatism in our opinions, and advised us to guard against generalizations in regard to prehistoric monuments of countries which, though neighbors, yet offer from the archeologic point of view such profound differences.

PSYCHOLOGY.

Note on the Evolution of the Upright Tail in the Domestic Dog.—My attention has recently been called to the work on "Organic Evolution" by Professor Eimer, page 114, to a paragraph in which he seems unable to account for the dogs in Constantinople carrying the tail upright. In speaking of the subject he says: "But the reason why these dogs begin to erect the tail and carry it upright, while the ancestral jackal, like the wolf, carries it hanging down, is not so easy to discover, although the fact could scarcely be explained as a case of adaptation." I beg to offer a provisional explanation of this phenomenon, and also to take exception to the latter statement, —that it cannot be explained as a case of adaptation. While my observations were not made at Constantinople, the dogs accompanying the several tribes of Indians I observed in the Western United States,[1] some of which are tamed wolves, or are directly descended from the wild American wolf known as the coyote, offer opportunities of study which brought me to a realization of this subject, which may be summed up in a few epitomized remarks.

As the dog becomes domesticated it is prone to use the tail as an organ of expressing mental states, especially those of emotion · for example, the wag of the tail expressive of delight, or sudden dropping of the tail between the legs at some disappointment or fright. The ancestral or wild wolf carries the tail hanging down, because that position (the tail being especially bushy and large in the wild animal) would be less conspicuous and more compatible with life in a free state of nature, or, as it were, to better elude detection. A family of wolves playing together undisturbed occasionally carry their tails curled upwards. By degrees the tail acquires naturally the upright position as a result of coincident evolution of the mind of the wolf incidental to domestication, and moreover thus instancing the slow adaptation of the appendage as an organ of expression. The cessation of natural selection in the domestic dog would give to the tail greater freedom of motion without detriment to life; and artificial selection perfects the caudal appendage into many diverse shapes. Still greater influence is exerted by certain expressions of the mind by that appendage, tending to keep it up, and by the influence of heredity, transmitting those tendencies. The muscles correlatively become strengthened and developed, and the erect position ultimately passes into an apparently fixed character in some varieties of the dog.—Dr. Joseph L. Hancock, *July 10th, 1891.*

[1] The words dog and wolf are used synonymously in this note.

MICROSCOPY.[1]

Fixation of the Methylenblue Stain.—In the last number of the *Zeitschrift für Wissenschaftliche Mikroskopie* (Vol. VIII., 1, 1891, p. 15), Prof. Dogiel offers some new points on the use of methylenblue in staining nerves.[2]

Nerves exposed to the direct action of methylenblue often stain so intensely that they appear dark blue, almost black. Such preparations should lie in the fixing medium (saturated aqueous solution of picrate of ammonium) twenty-four hours or more, otherwise the color fades quickly after transfer to glycerine. Long exposure to sunlight causes the stain to fade.

The fixing medium often macerates, loosening the epidermis and rendering difficult the investigation of intra-epithelial nerves. The macerating effects may be checked by adding osmic acid (1-2 cc. of a 1 per cent. solution to 100 cc. of the fixing fluid). This mixture preserves the tissue, and at the same time blackens the medullary sheaths of the nerve-fibers. As the axis cylinders stain dark violet, it becomes easy to distinguish medullated from non-medullated fibers. The preparations are mounted in dilute glycerine.

If the tissues are to be prepared for sectioning, a stronger per cent. of osmic is added (1-2 cc. of a 1 per cent. solution to 25–30 cc. of picrate of ammonium). The preparation lies in this fluid twenty-four hours, and is then cut in liver or pith, or with the freezing microtome.

Biedermann [3] recommends as a fixative in the case of invertebrates the following mixture:

Saturated aqueous solution of picrate of ammonium . . . . 1 vol.
Glycerine . . . . . 1 vol.
Solution of salt, ½ per cent. (sea-water in case of marine forms) 2 vols.

The nervous system is dissected out for exposure to the staining fluid. That the exposure may be as direct as possible, the nerve-cord (Hirudo, Lumbricus, etc.) should be freed from its sheath. A very dilute solution of methylenblue is used, and allowed to act from two to three hours. If the preparation, after the exposure, be placed on a slide wet with the staining fluid, and left for one-half to one hour in a moist chamber, the color effects will be intensified. This

[1] Edited by C. O. Whitman, Clark University. Worcester, Mass.

[2] Cf. this journal, Dec. 1890, p. 1219.

[3] *Jenaische Zeitschrift für Naturwissenschaft*, XXV., 3 and 4, 1891, p. 433.

"airing" is not required with marine animals, as with them the color differentiation is completed in the staining fluid.

In the case of marine animals the methylenblue is dissolved in sea-water. It dissolves less readily than in fresh water, and owing to this weak solubility it is liable to form a fine granular or crystalline precipitate on the surface of the preparation. As a large part of the dissolved staining substance is lost by filtering, it is best to prepare it fresh each time, and to allow it to settle, so that the clear fluid can be turned off for use. In the case of Nereis the nerve-cord is not obscured by a thick opaque sheath, and hence it is only necessary to open the dorsal wall lengthwise and spread it out flat in order to apply the stain.

Vasale's Modification of Weigert's Method.[4]—The pieces of the nervous system to be prepared are hardened in Müller's fluid or in bichromate of potash, and then, either with or without washing, left in alcohol until they are wanted for sectioning. For staining, the three following fluids are required:

1. Hæmatoxylin 1 g., dissolved in 100 g. water by heating.
2. Neutral acetate of copper, saturated, filtered solution.
3. Borax 2 g., ferricyanide of potash 2½ g., dissolved in 300 g. water.

The sections taken from alcohol are placed in solution 1 for three to five minutes; then for the same time in solution 2, in which they become black. They are next washed quickly in water and put into solution 3, which is stirred, and in which the ganglion-cells, neuroglia, and the degenerated parts are quickly discolored, while the medullated fibers remained stained dark violet. Finally the sections are washed in water and quickly placed in absolute alcohol. They are cleared with *carbol-xylol* (three parts xylol to one of liquid carbolic acid), and mounted in xylol balsam. This clearing mixture has the advantage that it does not shrink sections inclosed in celloidin. A contrast-stain may be obtained if the sections, after being washed, are treated with alum carmine or picrocarmine, or according to Pal's method.

Upson's Gold-Staining Method for Axis-Cylinders and Nerve-Cells.[5]—Pieces of the central nervous system are hardened in potassium bichromate, beginning with one per cent. and increasing to 2½ per cent. They are left in this fluid in the dark from four to six

[4] Rivista sperimentale di Freniatria e di Medicina legale, Vol. XV., 1889., p. 102–5. *Zeitschr. f. Wiss. Mikr.*, VII., 4, 1891, p. 518.

[5] A. Mercier. "Die Upson'schen Methoden für Achsencylinder und Zellen (Gold) Färbung. *Zeitschr. f. Wiss. Mikr.*, VII., 4, p. 474, 1891.

months, then washed quickly and placed in alcohol of 50 per cent. two to three days, which should be renewed a few times. They are then transferred to 95 per cent. alcohol, and left until they show plainly a greenish color (two to four weeks or more).

The preparations are then ready for sectioning, and may be cut without imbedding, or after imbedding in celloidin. Two methods of staining are used:

1. The section is placed in a one per cent. solution of gold chloride, to which two per cent. hydrochloric acid has been added (about two hours), then washed in distilled water, and transferred to a ten per cent. solution of potash for half a minute.

The section is then washed in distilled water, and there left until the following reducing fluid (to be made fresh each time) is prepared:

Acid. sulfurosum . . . . . 5 cc.
Tinct. iodi., 3 per cent. . . . . 10–15 drops.

Mix, and add

Liq. ferrichlorid. . . . . 1 drop.

This fluid must be made very exactly and quickly, and the moment it is ready the section is placed in it and left until it assumes a rose color. If left only a few seconds too long it becomes dark red, and is worthless.

As soon as the proper color appears the section is transferred to distilled water, and then to absolute alcohol on the slide. After ten to fifteen minutes it may be cleared in clove oil and mounted in balsam. The slide is to be kept in the dark.

2. The section is placed for two hours in the following solution:

Gold chloride, 1 per cent. . . . . 5 cc.
Ammonium vanadicum, saturated solution . . 10 drops.
Acid hydrochlor. . . . . 3 drops.

Wash, and transfer to the following (freshly prepared) mixture for ½ to 1 minute:

Caustic potash, 10 per cent. . . . . 5 cc.
Ammonium vanadicum . . . . . a trace.
Permanganate of potash, 10 per cent. . . . . 10 drops.

Wash, and expose to the following reducing fluid, freshly prepared:

Tin solution . . . . . 15 drops.
Aq. dest. . . . . 3 cc.
Iron solution . . . . . 3–5 drops.
Acid. sulfurosum . . . . . 3 cc.

The moment the acid sulfurosum is added a thick precipitate arises, and at this moment the reducing fluid is strongest.

The method of handling the section is the same as in method 1.

The tin solution is made by adding so much chloride of tin to three per cent. tincture of iodine until the color is white or yellowish. The iron solution is a saturated solution of ferrum phosphoricum in distilled water.

The method is somewhat cumbersome, but the results are said to be extraordinary.

Method of Preparing Rotifers.[6]—The first difficulty which one experiences in studying the rotifers is their constant motion. This difficulty is overcome, according to Masius, by the use of a mixture of methyl alcohol, water, and cocaine in weak solution.

After being anæsthetized by this fluid, the rotifers may be fixed without contraction in the ordinary preservative fluids,—Flemming's fluid, for example.

For the study of the head, an anæsthetized specimen is placed upon a slide, and the head cut off in a transverse plane as near as possible to the anterior end. The section thus obtained can be examined easily from any side in water or weak alcohol.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Natural Science Association of Staten Island.—April 11th.—Mr. Arthur Hollick stated that a nest of the barred owl (*Syrnium nebulosum*) was found on Staten Island, March 27th, by Mr. Chas. Rufus Harte, a student of Columbia College, This is an addition to the list of birds known to breed here, and the following memorandum from Mr. Harte was read:

"In the woods which are the continuation of the swamp (near Bull's Head) I came upon a sweet gum, having an 'owlish' looking cavity. As I turned aside to investigate, a barred owl flew out and away into the depths of the woods, appearing again once or twice, but always at a very respectable distance. The tree was about two feet in diameter, with no limbs below the opening, which was some thirty feet up, and very irregular in form. The cavity into which it opened was about eight inches in diameter, and was filled to within six inches of the mouth with dead leaves and feathers. On this bed lay the three

[6] *Arch. de Biol.*, X., 4, 1890, p. 652.

eggs, which were nearly hatched and very dirty. I did not see any remains of birds or mammals either in or about the nest."

May 9th.—A communication was read from Mitchell's Book Co., 830 Broadway, N. Y., stating that they had the original deed of sale of Staten Island by the Indians, in 1670, and inquiring if the association desired to purchase or copy it. The corresponding secretary stated that he had requested Mr. E. C. Delavan, Jr., to examine the document, with the following result:

56 Wall Street, New York, May 9th, 1891.

ARTHUR HOLLICK, Esq., New Brighton, N. Y.:

DEAR SIR:—I have examined the conveyance in possession of the Mitchell Company, 830 Broadway, referred to in their letter to you. While the document presents many internal evidences of authenticity, two points strike the professional reader as odd. In the recital of parties the names of the grantees are first written, followed by the names of the grantors. The grantees are Governor Lovelace and James Duke of York, the former representing the latter. The grantors are various sachems.

The second point that seems to me unusual is that no totems have been drawn by any of the sachem grantors, and in their place are the ordinary marks that would likely be adopted by any illiterate.

Granting the authenticity of the document, what is its value? The price placed on it by the company is $600. Its highest interest attaches when it is viewed from a purely antiquarian standpoint. Historically its interest is secondary. The first grant of Staten Island to Michael Pauw (1629–1630) was conditional on his acquiring the Indian titles, which we must assume that he accomplished. Pauw subsequently reconveyed to the West India Company (see Gay's History). After the English ousted the Dutch authorities a conveyance by the Indians of Staten Island to Governor Lovelace, before 1760, is said to have been made, followed by a deed of confirmation in 1760 (see Clute). The latter is probably the same instrument now under consideration.

From the lawyer's standpoint Indian deeds are now of little or no practical importance. It has been held that the Indians had no title which would be recognized in the courts of this country. The only legally recognized title was that of discovery and conquest. (Trustees of the Freeholders and Commonalty of the Town of Southampton, respondents, *vs.* The Mecox Bay Oyster Company, 116 N. Y. Johnson *vs.* McIntosh & Wheat (U. S.), 543. Martin *vs.* Waddell, 16 Peters 367.)

Very truly yours,

EDWARD C. DELAVAN, JR.

Mr. Arthur Hollick read by title a list of 35 fungi collected at Tottenville, October 4th, 1890, and determined by Chas H. Peck, State Botanist. This will be published as a "special" at some future date.

Mr. Hollick presented a specimen of *Spirophyton caudagalli*, found on the shore of Tottenville,—an addition to the local list of paleozoic fossils found in the Drift.

Mr. L. P. Gratacap showed specimens of *Lymnæa palustris*, and read the following memorandum: The *Lymnæa palustris*, which was found last autumn by Mr. Davis in the brook that courses along Washington Avenue, and which was identified by Mr. Sanderson Smith, has been kept in confinement by me during the winter. The tank in which the individuals were placed was kept in a very cold room and partook of of the changes in the winter weather. Two only survived the experience, and these have not hibernated, but maintained a sluggish life all winter. This spring seven gelatinous capsules exuded, each containing about twenty-five embryos. Amongst the authorities the opinion seems entertained that adults do not generally live over the winter, and that maturity is reached in one year. This opinion seems very questionable. The species may repay some attention. On this continent it ranges as far north as Great Bear Lake in Canada, and in the United States extends from New England through Pennsylvania and Kansas to California and Oregon. Abroad it ranges from Siberia to Algeria and Sicily. About five varieties are recorded by writers. The black patches of Lymnea upon the cement blocks just under the overflow from the new pond recently made in the Snug Harbor clearings, south of Castleton Avenue, may also prove to be this species.

Mr. W. T. Davis noted the Carolina wren, as an addition to the list of birds known to breed on the island, and read the following note:

On the 26th of last April I discovered a family of Carolina wrens (*Thryothorus ludovicianus*) on Richmond Hill near the old British fort. One of the parent birds was perched on top of a small Ailanthus tree calling vociferously, while the other accompanied the young, which were hidden in a thick growth of low briers, grass, etc., in and out of which they crept. They were just able to fly,—indeed, one of them could only do so for a yard or two, and much preferred climbing about the briers. Later in the day the little birds had congregated under a small cedar, whose lower branches touched the ground, but they quickly sought the protection of the briers again when approached. It is hoped that they will not be molested, but continue to abide on Staten Island,

for the Carolina wren remains all the year round where once it has fixed its home.

Mr. Davis also contributed the following botanical notes:

A swamp of three or four acres lies just north of the Amboy road, between Gifford's and the road to Richmond. At present it supports a thick growth of huckleberry bushes, poison sumachs, young red maples, a number of magnolias, etc. Several bushes of the mountain holly (*Nemopanthes canadensis*) also grow there, which species has not before been reported from the island. In July, 1889, the deep red berries were conspicuous; in 1890 the bushes bore no fruit; but on the 26th of April, this year, they were found in blossom. (Specimens were here shown.)

The peat is particularly thick and quaking in this swamp, and fifteen or twenty years ago, before it had been drained so extensively, the pitcher plant (*Sarracenia purpurea*) grew in its northwest corner, as I was informed by a man who lived in the vicinity. The common cranberry also grew there, and the man who told me about the pitcher plant, said his mother used to pick them for family use, but in his time he had never gathered over a handful. Now they appear to be exterminated. There is, however, an unreported patch of cranberries (*Vaccinium macrocarpum*), or perhaps more properly several patches, in the low, open woods between Washington Avenue and the road from Annadale.

SCIENTIFIC NEWS.

There has been no little change among biologists in the United States during the present year. The opening of the Leland Stanford, Jr., University made a number of new positions. Dr. D. S. Jordan, the president of Indiana University, was called to the presidency of the new institution, and so far he has announced the following natural science faculty: J. C. Branner, of Indiana University, professor of geology; O. P. Jenkins, of De Pauw University, professor of physiology and histology; J. H. Comstock, of Cornell, non-resident professor of entomology; C. H. Gilbert, of Indiana University, professor of vertebrate zoology; D.H. Campbell, of Indiana University, professor of cryptogamic botany. Prof. J. M. Coulter, of Wabash College, accepted the presidency of Indiana University; C. H. Eigenmann is appointed professor of zoology, and David Mottier instructor in morphological botany, in Indiana University. Prof. L. M. Underwood, of Syracuse University, goes to De Pauw to take the place vacated by Dr. Jenkins; and Prof. C. H. Hargitt and Mr. O. F. Cook are promoted to the chairs of biology in Syracuse University. Dr. Bleile has been elected professor of comparative anatomy and physiology in Ohio State University. Professor H. F. Osborn, of Princeton, has been elected professor of vertebrate zoology, and Prof. E. B. Wilson, of Bryn Mawr, associate professor of invertebrate zoology, in Columbia College. Prof. E. H. Barbour, of Iowa College, has been appointed associate professor of geology in the University of Nebraska; and H. W. Norris goes to Iowa College as professor of natural history.

Dr. Franz von Wagner is a privat docent in the University of Strassburg.

Peter Martin Duncan, the well-known student of fossil Cœlenterates and Echinoderms, died in London, May 28th, 1891.

Professor Chun's place in Königsberg is taken by Dr. Max Braun, of Rostock, while Dr. Fr. Blochman goes to the zoological chair in Rostock.

Edmond André, the student of the Hymenoptera, died at Braune, January 11th, 1891.

Dr. A. Viallanes has been appointed director of the zoological station at Arcachon, France.

Edward Burgess died in Boston, July 12th, 1891. He was born in Sandwich, Mass., in 1848, graduated in 1871 from Harvard University, and was for several years professor of entomology in the Bussey Institution of Harvard. He was secretary and librarian of the Boston Society of Natural History from 1873 to 1883, when he resigned to devote himself to naval architecture. Mr. Burgess described some Diptera, but the bulk of his work was on the anatomy of insects. He was a careful dissector and a good artist, his "Anatomy of the Milkweed Butterfly," published in 1881, being a good example of what anatomical work should be.

J. M. Stedman, formerly of Cornell University, now of the U. S. Department of Agriculture, has just accepted an invitation to the chair of biology in Trinity University, Durham, N. C. This institution has been completly reorganized, and will open in September with the following new departments: Medical college, law school, schools of arts, literature, political and social science, divinity, and a college of the sciences.

The marine biological laboratory at Wood's Holl, Mass., is enjoying its most prosperous season, some 54 students and investigators being at work at present. The Gifford House, purchased last year, makes fine quarters for the mess, while the Fay and Gardner cottages relieve the pressure for suitable rooms.

Professor W. K. Brooks, with a party of students, went to Jamaica for the summer. The party report good results, but expensive living. Outrageous prices were charged for every building available for laboratory purposes.